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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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In Mr. Sturgeon's Paper, p. 231, *for* magnetic electrometer *read* magnetic electromotor.

THE

LONDON AND EDINBURGH

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

JULY 1835.

I. *On the Gold-workings formerly conducted in the County of Wicklow, Ireland.* By THOMAS WEAVER, *Esq., F.R.S. F.G.S. M.R.I.A. &c. &c.**

SINCE my return to Europe, after an absence of nearly three years, among the scientific publications that have attracted my attention, the Journal of the Geological Society of Dublin claimed particular notice. I rejoiced to perceive a growing spirit of geological inquiry in Ireland, and more especially as that spirit is understood to have been mainly excited and promoted by the Trigonometrical Survey of the island now in progress under the able conduct of Colonel Colby, R.E.

It was, however, with surprise and regret that I read the following passage in the Address delivered at the First Annual Meeting of that Society by their President. Speaking of the natural resources of Ireland, the learned author says: "Even native gold was found not many years ago in a neighbouring county in large wedges. It was discovered in a limited portion of a stream of about one hundred yards in extent; and though the stream was repeatedly examined up to the mountain from which it descended, not a trace of the metal could be perceived in any other part; yet, notwithstanding this circumstance, the mountain itself was presumed to be the chief depository: it was accordingly perforated quite through, with great labour and at an enormous expense; and the search conducted in this way having proved unsuccessful,

* Communicated by the Author.

was finally abandoned. The limited portion of the stream in which any gold could be discovered, we might suppose, should have directed to the adjacent banks, and the hills which rise from the spot on either side. These might have been explored at much less expense; and judging by the uncommonly great size of some of the lumps which had been found, it seems not unreasonable to expect, notwithstanding the past disappointment, that much will hereafter be brought to light."

A judgment thus gravely pronounced from a presidential chair demands attention. From what source the deliverer of that sentence drew his information does not appear; yet certainly a more strange misapprehension of facts has seldom been embodied in words. In justice to the Government under whose auspices I had the honour to act, as well as to the memories of the individuals concerned in directing the operations (of whom I am the only survivor), it becomes incumbent on me to disabuse the public by recalling their attention to the real circumstances of the case; and this I might briefly do by a simple reference to the succinct and faithful history I have given of the proceedings in my memoir on the East of Ireland, published in the 5th volume of the Geological Transactions, First Series. But as those Transactions are, comparatively speaking, in the hands of few persons, it appears expedient to enter into some details, through the medium of a scientific Journal of extensive circulation; for which purpose I subjoin the following extracts from my memoir on the East of Ireland, merely transposing a few passages for the sake of greater distinctness*.

"§§ 105. 106. The discovery of native gold in the Ballinvalley stream at Croghan Kinshella mountain was accidental; and, as it subsequently proved, the principal depôt of the gold in that stream extended about twelve hundred yards below the ford. In other quarters the gold obtained was comparatively of small amount. The largest piece found in any of the other streams was on the Coolbawn side. It weighed $2\frac{1}{2}$ ounces. But in all these cases the gold was accompanied by most of the metallic substances that occurred in the Ballinvalley stream, and which will be enumerated hereafter. Under the Act of Parliament for managing the undertaking, the directors established regular stream-works; and up to the unhappy period of the rebellion in May 1798, when the works

* The publication of this article has been delayed in the hope of recovering the plate of the Geological Map of Croghan Kinshela mountain, which would have thrown a clearer light on the subject; but as, unfortunately, that plate is not forthcoming, I must necessarily refer in this respect to the 5th volume of the Transactions of the Geological Society, First Series.

were destroyed, Government had been fully reimbursed its advances, the produce of the undertaking having defrayed its own expenses, and left a surplus in hand. In the year 1801, the operations were resumed, when the directors proposed to Government not to confine its views to the mere collection of the alluvial gold, but to extend the researches, directing them more particularly toward the discovery of auriferous veins. They were justified, as they conceived, in this proposition by the following considerations:

"1. The well-known fact, that in the various quarters of the world, in America, Africa, Asia, and Europe, auriferous veins often occur in the mountains adjacent to the districts that abound in massy alluvial gold*.

"2. The peculiar circumstances under which the native gold of Croghan Kinshela was found.—It occurred in massy lumps, and in small pieces, down to the minutest grain. One piece weighed twenty-two ounces, another eighteen ounces, a third nine ounces, and a fourth seven ounces. The gold was found, accompanied by other metallic substances, dispersed through a kind of stratum composed of clay, sand, gravel, and fragments of rock, and covered by soil, which sometimes attained to a very considerable depth, from twenty to fifty feet, in the bed and banks of the different streams. By the operations in the stream-works, (which are too well known to professed miners to need detail on the present occasion,) all the metallic particles thus disseminated were collected in a concentrated mass; and in those at Ballinvalley it was constantly found that the gold was attended by magnetic ironstone, sometimes in masses of half a hundred weight, by magnetic ironsand, by cubical and dodecahedral iron pyrites; and, in small pieces and grains, by specular iron ore, brown and red ironstone, iron ochre, fragments of tinstone crystals, wolfram, gray ore of manganese, pieces of quartz and chlorite, and sometimes fragments of quartz crystals. I observed and collected also some specimens, which show that the gold, magnetic ironstone, and wolfram were each of them frequently intermingled with quartz; and I have also a few specimens which exhibit the gold not only incorporated with iron ochre, but ramifying in slight threads through wolfram. Some of the gold, though very rarely, occurred crystallized in octohedrons, and also in

"* See, for example, the Travels of Ulloa and Humboldt in America; Molina's Chili; Park's Travels in Africa; Marsden's Sumatra; Turner's Thibet; *Abhandlungen über Bergbaukunde*; Dr. Clarke's Travels in Europe, Asia, and Africa, vol. iv.; Robilant on Piedmont, in *Journal des Mines*, vol. ix.; and D'Aubuisson in vol. xxix."

the elongated garnet dodecahedron, of which I retain specimens.

"3. The ascertained fact that several of the contemporaneous veins of quartz, contained in the mountain, were metalliferous, yielding magnetic ironstone, iron pyrites, copper pyrites, blende and iron ochre, with chlorite and quartz crystals.

"Under these considerations, the measures proposed by the directors were: to continue the stream-works, progressively advancing toward the head of the several streams; to examine more closely the solid mass of the mountain, by means of trenches to be cut in various directions down to the firm rock; to explore more fully the veins already known, and those which might be discovered near the surface by the trenches; and lastly, to try these veins, at a considerable depth from the surface, by means of a gallery or level, to be driven into the mountain, in a direction nearly at right angles with the general range of the veins, selecting that quarter where they appeared to be most numerous.

"These measures receiving the sanction of Government, they were carried into effect; and numerous trials were made by driving and sinking on the veins previously known and subsequently discovered. By the Ballinvalley trench alone twenty-seven veins of quartz were found, varying from nine inches to four feet wide, in a distance of 700 fathoms; and in the same manner, by the Ballinagore trench, eighteen quartz veins were discovered in a distance of 600 fathoms. These veins partly range and dip with the slaty rock, but they also ramify and terminate in strings which intersect the latter. They are evidently contemporaneous, and most of them are barren, but some were found bearing magnetic ironstone, iron pyrites, and iron ochre, with chlorite. Two of the most powerful veins occur on the western side of the northern arm of the mountain, one being six feet wide in the broadest part, and the other four feet. They consist principally of massive magnetic ironstone and quartz, with disseminated copper pyrites, and iron pyrites. A similar vein, four or five feet wide, with the addition of blende, occurs near the extremity of the northern arm, and another, two and a half feet wide, appears in the face of the rock over the Daragh river." In the Geological Map of Croghan Kinshela mountain *, it may be seen that the level driven into the mountain, nearly at right angles

* See Plate XI. of the 5th volume of the Geological Transactions. That Map serves not only to elucidate the geological relations of the mountain, but also to explain the nature of the mining operations which were carried on there; the whole being laid down from actual survey.

with the general range of the veins, extended 178 fathoms in length.

"The mineral substances obtained by these various operations were subjected to experimental processes both by fire and amalgamation; but in no instance was a particle of gold elicited from them either by the one or the other process. So unsatisfactory a result led to the persuasion, that the gold formed no part of the veins which appear in the mountain; and hence Government were induced to abandon the enterprise.

"The same conclusion seems to apply to the tinstone, wolfram, and manganese, in discovering which the mining operations equally failed. What is, then, the primary source of these substances found detached in the beds and banks of the streams? It is not improbable that they occur more or less scattered and disseminated through the rocks of the mountain, although it must be acknowledged that no discovery leading to this inference attended the researches of Government. It is, however, well known, that such occurrences are not uncommon in other tracts*; and we may, then, conceive the gold and the other metallic substances to have been detached from their native seat, and to have been lodged in their present position, in company with other alluvial materials, at the time of the first formation of soil, upon the retrocession of the ocean.

"The total quantity of native gold collected by Government amounted to 944 ounces 4 pennyweights and 15 grains, producing when sold 3675*l.* 7*s.* 11$\frac{1}{2}$*d.*; the ingots having yielded from 21$\frac{3}{8}$ to 21$\frac{7}{8}$ carats of fine gold, the alloy being silver."

"§ 107. But the discovery of native gold was not confined to Croghan Kinshela. Trials were instituted by the directors in another mountain also, namely Croghan Moira, about seven miles distant from the former mountain, and gold was obtained there, though in very small quantity, the largest piece not exceeding two and a half pennyweights in weight. One trial was made on Ballycreen in the stream at the eastern foot of the mountain, and minute particles of gold were found, accompanied by magnetic ironstone, magnetic ironsand, compact brown ironstone, cubical iron pyrites, and numerous small garnets. Another trial was made on the western side

"* See, for example, Ulloa's Travels in South America, vol. i i. p. 160, English translation; Humboldt's Political Essay on New Spain; Charpentier's *Beobachtungen über die Lagerstätte der Erze*; Von Buch's *Geognostische Beobachtungen auf Reisen durch Deutschland und Italien,* vol. i. p. 128—130."

of the mountain, in Ballynacapogue brook, and small particles of gold were obtained, with magnetic ironstone, magnetic ironsand, and fragments of tinstone crystals. A third trial, in Fannanerin stream, on the north-eastern flank of the mountain, did not produce any gold."

The preceding extracts may serve to show how untenable are the positions advanced in the statement of the distinguished President. They prove,

1. That the discovery of native gold was not confined to a limited portion of a stream of about one hundred yards in extent.

2. That gold was found in other parts, and in other streams proceeding from the same mountain; and indications to this effect are given in the Map of Croghan Kinshela mountain.

3. That the mountain was not perforated through, nor with great labour, nor at an enormous expense. And the sole object of a level, which was driven 178 fathoms long into the mountain, was to try in depth veins found by trenching at the surface. The moderate expense of such a level passing through a clayslate rock (and that rock dipping toward, and thus favouring the progress of the level,) any competent miner may readily compute.

4. That the adjacent banks and hills rising on either side were attended to; for in fact the mountain was scarified by trenches on every side, cut down to the live rock; and many veins were discovered in consequence, upon which workings were conducted, and the produce subjected to repeated experiments. These veins were thus explored at the least expense.

With this plain statement of facts I forbear comment, merely adding that I am persuaded that few, if any, Government undertakings have ever been conducted in a more œconomical manner, or at so little charge to the public. The great object was to determine, according to the approved principles of practical miners, whether auriferous veins existed in the mountain; and the negative appearing to be proved, Government were induced to relinquish the enterprise. Prior to the operations of Government, my friends had joined me in a proposition to embark in the adventure ourselves, yielding to the Crown one fifth part of the produce. Apprehensive that the subject might become too rich, that proposition was rejected. The result of the subsequent researches, however, gave us reason to be content that our offer had not been accepted by Government.

II. *On* Olbers's *Method of determining the Orbits of Comets. By Professor* Encke.*

Dr. OLBERS had the kindness to transmit to me last year the preceding paper, and to permit me to insert it in this volume. It completes his excellent method of determining the orbits of comets in a point in which greater facility and a quicker approximation to the truth by trials more methodically conducted had been desired by many of those who employed this method, and it finishes in some measure the structure, the foundation of which Olbers laid in the year 1797.

Dr. Olbers supposes in this addition that his readers are acquainted with his former formulæ. Although this method was already carried to such a degree of perfection in the first memoir†, that even the master-hand of the author of the *Theoria motus*, &c., made no essential alterations in it, but only some abbreviations, and although consequently the subject might be considered as exhausted, yet I hope I shall not incur censure if I once more completely explain this method of Dr. Olbers. The development of the necessary formulæ will give rise to some remarks, partly regarding the accuracy of the method in general, partly referring to the above important addition lately made to it by Olbers himself. Those remarks are indeed not new, but their being placed together in this publication will, I trust, by no means be deemed improper.

Lambert's theorem is a main part of Olbers's method. The manner of solving it given by Olbers admitting of some abbreviations, I shall begin with explaining this little improvement.

Denoting two radii vectores of a parabola, the minimum focal distance of which is q, by r, r', the corresponding true anomalies by v, v', and the interval of time between them by t, the double area described will be by Kepler's laws,

$$F = K t \sqrt{2 q},$$

where K is the constant of the *Theoria motus*:

$$K = 0{\cdot}0172021.$$

On the other hand we have

$$F = \int r^2 \, d v, \text{ from } v = v, \text{ to } v = v'.$$

Substituting for r, its value, $\quad r = \dfrac{q}{\cos \frac{1}{2} v^2}$

* Encke's *Jahrbuch* for 1833, page 264.

† Memoir on the most easy and most convenient Method of Calculating the Orbit of a Comet, by William Olbers. Weimar 1797.

we have, by integration,

(1) $\quad Kt\sqrt{2q} = 2q^2(\tan\frac{1}{2}v' - \tan\frac{1}{2}v + \frac{1}{3}\tan\frac{1}{2}v'^3 - \frac{1}{3}\tan\frac{1}{2}v^3)$.

Introducing instead of the true anomaly and the smallest distance, the radii vectores and the chord between, we obtain the important advantage of exterminating the elements of the parabola from the equation. In applying the method of co-ordinates it is likewise more convenient to retain distances only in the formula. With this view put

$$(2) \qquad v' - v = 2f,$$

and taking k for the chord, we have

$$k^2 = r^2 + r'^2 - 2rr'\cos 2f, \text{ or } = (r+r')^2 - 4rr'\cos f^2;$$

consequently

$$2\cos f\sqrt{rr'} = \pm\sqrt{(r+r'+k)(r+r'-k)}.$$

For the sake of brevity we make

$$(3) \qquad \begin{aligned} r + r' + k &= m^2 \\ r + r' - k &= n^2, \end{aligned}$$

which is permitted in every case, even with the condition that m and n shall be always positive; and we have

$$(4) \qquad \begin{aligned} r + r' &= \tfrac{1}{2}(m^2 + n^2) \\ 2\cos f\sqrt{rr'} &= \pm mn. \end{aligned}$$

The upper sign for which, $f \angle 90°$, $v' - v \angle 180°$, may be almost considered as the only one here to be referred to.

By the same quantities $\sin f$ may likewise be expressed. For we have $\sin f^2 = \sin\frac{1}{2}(v' - v)^2$

$$= \cos\tfrac{1}{2}v^2 + \cos\tfrac{1}{2}v'^2 - 2\cos\tfrac{1}{2}(v'-v)\cos\tfrac{1}{2}v\cos\tfrac{1}{2}v';$$

and by introducing r and r' we obtain

$$\sin f^2 = \frac{q}{r} + \frac{q}{r'} - 2q\frac{\cos f}{\sqrt{rr'}}$$

$$= q\,\frac{r + r' - 2\cos f\sqrt{rr'}}{rr'},$$

and by substitution from (4),

$$(5) \qquad 2\sin f\sqrt{rr'} = (m \mp n)\sqrt{2q},$$

where, as above, the upper sign is to be taken for $f \angle 90°$, and the lower one for $f > 90°$.

Separating in (1) the factor contained in the part on the right-hand side we have,

$$F = Kt\sqrt{2q} = 2q^2(\tan\tfrac{1}{2}v' - \tan\tfrac{1}{2}v)$$
$$(1 + \tfrac{1}{3}\tan\tfrac{1}{2}v'^2 + \tfrac{1}{3}\tan\tfrac{1}{2}v'\tan\tfrac{1}{2}v + \tfrac{1}{3}\tan\tfrac{1}{2}v^2);$$

and placing in this equation instead of 1, its value deduced from this equation,

$$1 + \tan \tfrac{1}{2} v' \tan \tfrac{1}{2} v = \frac{\cos \frac{1}{2} (v' - v)}{\cos \frac{1}{2} v' \cos \frac{1}{2} v} = \frac{\cos f \sqrt{r r'}}{q},$$

and likewise for the one factor its value, viz.

$$\tan \tfrac{1}{2} v' - \tan \tfrac{1}{2} v = \frac{\sin \frac{1}{2} (v' - v)}{\cos \frac{1}{2} v' \cos \frac{1}{2} v} = \frac{\sin f \sqrt{r r'}}{q},$$

we obtain

$$(6) \quad \mathrm{F} = \mathrm{K} t \sqrt{2 q} = 2 r r' \sin f \cos f + \tfrac{2}{3} \frac{(r r')^{\frac{3}{2}}}{q} \sin f^3,$$

or after substitution from (4) and (5),

$$\mathrm{K} t \sqrt{2 q} = \pm \tfrac{1}{2} (m \mp n) m n \sqrt{2 q} + \tfrac{1}{3} \frac{(m \mp n)^3}{\sqrt{2}} \sqrt{q},$$

therefore

$$2 \mathrm{K} t = \pm (m \mp n) m n + \tfrac{1}{3} (m \mp n)^3 = \tfrac{1}{3} (m^3 \mp n^3),$$

or

$$(7) \qquad (r + r' + k)^{\frac{3}{2}} \mp (r + r' - k)^{\frac{3}{2}} = 6 \mathrm{K} t.$$

This equation is solved by trials in Olbers's method. Approximate values of k, r, r' are substituted until the equation is satisfied. As it is evidently quite indifferent what form is given to it, we may just as well deduce the value of one of the variable quantities from it.

The powers being expanded in series, the odd terms will destroy one another, (having for the moment only regard to the upper sign,) and we shall have

$$6 \mathrm{K} t = 3 k (r + r')^{\frac{1}{2}} - \frac{1.3}{4.6} k^3 (r + r')^{-\frac{3}{2}} - \frac{1.3.3.5}{4.6.8.10} k^5 (r + r')^{-\frac{7}{2}} \text{ \&c.}$$

or

$$\frac{2 \mathrm{K} t}{(r + r')^{\frac{3}{2}}} = \frac{k}{r + r'} - \frac{1}{4.6} \left(\frac{k}{r + r'}\right)^3 - \frac{1.3.5}{4.6.8.10} \left(\frac{k}{r + r'}\right)^5 - \text{\&c.}$$

Reversing this series, so that the value of $\frac{k}{r + r'}$ is determined from it, and putting for brevity

$$(8) \qquad \frac{2 \mathrm{K} t}{(r + r')^{\frac{3}{2}}} = \eta, \text{ we shall find}$$

$$\frac{k}{r + r'} = \eta + \frac{1}{24} \eta^3 + \frac{5}{384} \eta^5 - \frac{59}{9216} \eta^7, \text{ \&c.*}$$

or

$$(9) \quad k = \frac{2 \mathrm{K} t}{\sqrt{(r + r')}} \left\{ 1 + \frac{1}{24} \eta^2 + \frac{5}{384} \eta^4 + \frac{59}{9216} \eta^6 + \dots \right\}.$$

* Lambert's *Insigniores Orbitæ Cometarum Proprietates*, p. 63.

If we consider the intervals of time as small quantities of the first order, η will be of the same order. The factor of $\frac{2\,\mathrm{K}\,t}{\surd(r+r')}$ will, consequently, differ from unity only by quantities of the second and higher orders, and may be conveniently represented in a table with the argument η. The calculation is then very simple. The approximate value of r, r', k being given, $\frac{2\,\mathrm{K}\,t}{\surd(r+r')}$ is first calculated, from which η is immediately derived, and this gives by the table immediately the second factor. The comparison of the value of k thus found, with the one assumed, will prove how far that value is correct.

The value of this factor, which I will designate by μ, so that

$$(10) \qquad \mu = 1 + \frac{1}{24}\eta^2 + \frac{5}{384}\eta^4 + \frac{59}{9216}\eta^6 \ldots$$

may also be expressed by a finite formula, by which the calculation of the table is facilitated, and its use immediately extended to all cases.

We may always put $\frac{k}{r+r'} = \sin\gamma$, whence

$$\frac{6\,\mathrm{K}\,t}{(r+r')^{\frac{3}{2}}} = (1+\sin\gamma)^{\frac{3}{2}} \mp (1-\sin\gamma)^{\frac{3}{2}}.$$

Assuming, what we may assume, that γ always $\angle\, 90°$, we may use this formula,

$$(\cos\tfrac{1}{2}\gamma \pm \sin\tfrac{1}{2}\gamma)^2 = 1 \pm \sin\gamma,$$

and the above equation will be transformed thus,

$$(\cos\tfrac{1}{2}\gamma + \sin\tfrac{1}{2}\gamma)^3 \mp (\cos\tfrac{1}{2}\gamma - \sin\tfrac{1}{2}\gamma)^3 = \frac{6\,\mathrm{K}\,t}{(r+r')^{\frac{3}{2}}}.$$

Taking first the upper sign, we obtain

$$6\cos\tfrac{1}{2}\gamma^2\sin\tfrac{1}{2}\gamma + 2\sin\tfrac{1}{2}\gamma^3 = \frac{6\,\mathrm{K}\,t}{(r+r')^{\frac{3}{2}}},$$

to which this form may be given,

$$3\left(\frac{\sin\frac{1}{2}\gamma}{\surd 2}\right) - 4\left(\frac{\sin\frac{1}{2}\gamma}{\surd 2}\right)^3 = \frac{6\,\mathrm{K}\,t}{2^{\frac{3}{2}}\,(r+r')^{\frac{3}{2}}}.$$

Lambert's formula shows at once, as $k \angle (r+r')$, that in both cases also

$$6\,\mathrm{K}\,t \angle 2^{\frac{3}{2}}\,(r+r')^{\frac{3}{2}}.$$

Making, therefore,

$$\frac{6\,\mathrm{K}\,t}{2^{\frac{3}{2}}\,(r+r')^{\frac{3}{2}}} = \sin\theta,$$

we get from the last equation

$$\sin \tfrac{1}{2} \gamma = \sin \tfrac{1}{3} \theta \sqrt{2}.$$

Of the three roots of the cubic equation, one only fulfills the two conditions, that

$$\sin \tfrac{1}{2} \gamma < \sin 45^\circ \quad \text{or} \quad < \tfrac{1}{2} \sqrt{2},$$

and is positive, viz. the one for which θ has been taken $< 90^\circ$.

For the lower sign we have this form:

$$3 \left(\frac{\cos \frac{1}{2} \gamma}{\sqrt{2}}\right) - 4 \left(\frac{\cos \frac{1}{2} \gamma}{\sqrt{2}}\right)^3 = \frac{6 \, \mathrm{K} t}{2^{\frac{3}{2}} (r + r')^{\frac{3}{2}}},$$

or $$\cos \tfrac{1}{2} \gamma = \sin \tfrac{1}{3} \theta \sqrt{2}.$$

Here $\sin \frac{1}{3} \theta$ must be $< \frac{1}{2} \sqrt{2}$, and at the same time $\cos \frac{1}{2} \gamma > \frac{1}{2} \sqrt{2}$. These two conditions are only fulfilled when θ is between 90° and 135°. It follows from this that when the equation

$$\frac{6 \, \mathrm{K} t}{2^{\frac{3}{2}} (r + r')^{\frac{3}{2}}} = \sin \theta,$$

gives a value for θ which is $< 45^\circ$, for the same data only one solution is possible, for which $v' - v < 180^\circ$. But when $\theta > 45^\circ$, there are two solutions, θ and $180^\circ - \theta$, the latter of which answers to $v' - v > 180^\circ$.

The value of θ may be thus written:

$$\sin \theta = \frac{3 \eta}{\sqrt{8}};$$

and as we have both from the value of $\sin \frac{1}{2} \gamma$ as well as from that of $\cos \frac{1}{2} \gamma$

$$\sin \gamma = 2^{\frac{3}{2}} \sin \tfrac{1}{3} \theta \sqrt{\cos \tfrac{2}{3} \theta},$$

and likewise $$k = (r + r') \sin \gamma,$$

we have the following complete system of formulæ:

$$\eta = \frac{2 \, \mathrm{K} t}{(r + r')^{\frac{3}{2}}},$$

$$\text{(11)} \qquad \sin \theta = \frac{3 \eta}{\sqrt{8}}$$

$$\mu = \frac{3 \sin \frac{1}{3} \theta}{\sin \theta} \sqrt{\cos \tfrac{2}{3} \theta}$$

$$k = \frac{2 \, \mathrm{K} t}{\sqrt{(r + r')}} \, \mu,$$

where

$$\log 2 \, \mathrm{K} = 8{\cdot}5366114.$$

By means of these formulæ, $\log \mu$ has been calculated in the

annexed table for all values of η, from $\eta = 0$ to $\eta = \frac{1}{3}\sqrt{8}$, increasing by hundredth parts of unity. The logarithms by which these calculations were made being only correct to 7 decimals, the last figure may be wrong one or at most two units, which, however, will have the less influence, as Lambert's formula is in this way applied with much greater accuracy than in the common way. In the latter, as k is small in comparison of $r + r'$, a small quantity is obtained as the difference of two greater ones, while there is no such subtraction in the use of the table. The case of $v' - v > 180°$ has not been taken into consideration, as the interpolation between the values of $\log \mu$ would then be too tedious. Besides, in these rarer cases there is not the same disadvantage in following the common process.

Lambert's formula shows most clearly how many observations are required for determining the parabolic orbit. If the distance of a comet from the earth were known for one observation, its place in space, consequently likewise its place with regard to the sun, would be given. Every observation which is used introduces, therefore, one unknown quantity; for two observations two radii vectores and the connecting chord k might be expressed by two unknown distances, but only one equation between r, r' and k would be derived from them. But three observations would give three equations for three unknown quantities, by combining every two of the three radii vectores; this would be consequently sufficient for solving the problem. This application would satisfy one of Kepler's laws, viz. that the times are proportional to the areas described; but it is quite independent of the second equally important law, that the comet must move in a plane passing through the sun. This latter law gives for the three places of the comet an equation of condition, so that in three complete observations there are four equations with three unknown quantities. Although, therefore, two observations are not sufficient for determining the parabola, yet three observations are more than enough, and it will be necessary either not perfectly to satisfy one of the data contained in the three complete observations, or only to fulfill one condition resulting from a combination of two data while the other is entirely neglected.

Olbers's method is founded on the condition that the three places of the comet shall be in one plane with the sun. It determines from it with a close approximation the ratio of two distances from the earth, and consequently also two places of the comet in space, expressed by one unknown quantity. This ratio applied to Lambert's equation between the places and the time would give, if the substitution were actually made, one

equation with one unknown quantity, but of a very high degree, which would have to be solved by trials. It is therefore preferred to solve the system of those equations by trials.

If we denote by x, y, z, the coordinates of a point referred to the centre of the sun as the beginning of the coordinates, we have the following three equations for three points in the same plane passing through the centre of the sun:

$$\begin{aligned} A\,x + B\,y + C\,z &= 0 \\ A\,x' + B\,y' + C\,z' &= 0 \\ A\,x'' + B\,y'' + C\,z'' &= 0. \end{aligned}$$

The equation of condition for these nine coordinates, after the ratios $\frac{B}{A}$ and $\frac{C}{A}$ have been eliminated, may be represented in different forms useful for future application. In order to find the factors by which each of these equations must be multiplied with a view to eliminate B and C, by which means A likewise disappears, we have to combine these two equations:

$$\begin{aligned} p\,y + p'\,y' + p''\,y'' &= 0 \\ p\,z + p'\,z' + p''\,z'' &= 0. \end{aligned}$$

We obtain from them

$$\begin{array}{lll} p & \text{proportional to} & y''\,z' - y'\,z'' \\ p' & \text{———————} & y\,z'' - y''\,z \\ p'' & \text{———————} & y'\,z - y\,z', \end{array}$$

or as these latter values are the double areas of the triangles between the beginning of the coordinates and the points $y'\,z'$, $y''\,z''$, &c., respectively in the plane of the $y\,z$, and as these again are proportional to the areas of the triangles in the plane of the orbit, these factors become proportional to $[r'\,r'']$, $-[r\,r'']$, $[r\,r']$, if these symbols stand for the double areas of the three triangles, viz. sun, second and third places of the comet; sun, first and third places of the comet; sun, first and second places of the comets. The negative sign is introduced in order to avoid negative areas, in as much as all areas are supposed to originate from the turning of the first radius vector always in the same sense.

Hence the equation of condition has three forms:

$$(12)\qquad \begin{aligned} [r'\,r'']\,x - [r\,r'']\,x' + [r\,r']\,x'' &= 0 \\ [r'\,r'']\,y - [r\,r'']\,y' + [r\,r']\,y'' &= 0 \\ [r'\,r'']\,z - [r\,r'']\,z' + [r\,r']\,z'' &= 0. \end{aligned}$$

Let us now designate

The three curtate distances of the comet from the earth, by . . . . $\Big\}$ ϱ, ϱ', ϱ'',

The three observed geocentric longitudes of the comet, by	$\alpha,\ \alpha',\ \alpha''$,
The three geocentric latitudes of the comet, by .	$\delta,\ \delta',\ \delta''$,
The three longitudes of the sun, by	$\Theta,\ \Theta',\ \Theta''$,
The three distances of the sun from the earth, by	$R,\ R',\ R''$,
The three times of the observation, by	$t,\ t',\ t''$,

Hence we have

$$x = \varrho \cos \alpha - R \cos \Theta$$
$$y = \varrho \sin \alpha - R \sin \Theta$$
$$z = \varrho \tan \delta$$

and the analogous equations for the six other coordinates. Substituting these expressions in (12) we get,

$$[r' r''] (\varrho \cos \alpha - R \cos \Theta) - [r r''] (\varrho' \cos \alpha' - R' \cos \Theta') + [r r'] (\varrho'' \cos \alpha'' - R'' \cos \Theta'') = 0$$
$$[r' r''] (\varrho \sin \alpha - R \sin \Theta) - [r r''] (\varrho' \sin \alpha' - R' \sin \Theta') + [r r'] (\varrho'' \sin \alpha'' - R'' \sin \Theta'') = 0$$
$$[r' r''] \varrho \tan \delta - [r r''] \varrho' \tan \delta' + [r r'] \varrho'' \tan \delta'' = 0.$$

In these equations there are five unknown quantities, the three ρ, ρ', ρ'', and two ratios between the triangles. Two of them, one ϱ and one ratio, may be eliminated. As every one of these equations separately expresses one and the same condition, and as they are besides independent of the assumed position of the axis of abscissæ, it will be permitted to simplify them by changing the position of this axis. The most convenient forms are the following, which result from the former by changing the position of the axis of abscissæ, in the first of them by Θ', and in the second, first by α', and again by Θ', the third remaining unchanged.

$$[r' r''] \big(\varrho \cos (\alpha - \Theta') - R \cos (\Theta - \Theta')\big) - [r r''] \big(\varrho' \cos (\alpha' - \Theta') - R'\big) + [r r'] \big(\varrho'' \cos (\alpha'' - \Theta') - R'' \cos (\Theta'' - \Theta')\big) = 0$$

$$[r' r''] \big(\varrho \sin (\alpha' - \alpha) + R \sin (\Theta - \alpha')\big) - [r r''] R' \sin (\Theta' - \alpha') - [r r'] \big(\varrho'' \sin (\alpha'' - \alpha') - R'' \sin (\Theta'' - \alpha')\big) = 0 \qquad (13)$$

$$[r' r''] \big(\varrho \sin (\alpha - \Theta') + R \sin (\Theta' - \Theta)\big) - [r r''] \varrho' \sin (\alpha' - \Theta') + [r r'] \big(\varrho'' \sin (\alpha'' - \Theta') - R'' \sin (\Theta'' - \Theta')\big) = 0$$

$$[r' r''] \varrho \tan \delta - [r r''] \varrho' \tan \delta' + [r r'] \varrho'' \tan \delta'' = 0.$$

equations which likewise might have been obtained by the combination of the two first of the preceding ones.

Eliminating ϱ' from the two last, we obtain

$$[r'r'']\,\rho\,\big(\tan\delta'\sin(\alpha-\Theta')-\tan\delta\sin(\alpha'-\Theta')\big)$$
$$+\,[r'r'']\,\mathrm{R}\tan\delta'\sin(\Theta'-\Theta)$$
$$+\,[r\,r']\,\rho''\big(\tan\delta'\sin(\alpha''-\Theta')-\tan\delta''\sin(\alpha'-\Theta')\big)$$
$$-\,[r\,r']\,\mathrm{R}''\tan\delta'\sin(\Theta''-\Theta')=0,$$

whence

$$\rho''=\frac{[r'r'']}{[r\,r']}\cdot\frac{\tan\delta'\sin(\alpha-\Theta')-\tan\delta\sin(\alpha'-\Theta')}{\tan\delta''\sin(\alpha'-\Theta')-\tan\delta'\sin(\alpha''-\Theta')}\cdot\rho$$
$$+\frac{\tan\delta'}{[r\,r']}\cdot\frac{[r'r'']\,\mathrm{R}\sin(\Theta'-\Theta)-[r\,r']\,R''\sin(\Theta''-\Theta')}{\tan\delta''\sin(\alpha'-\Theta')-\tan\delta'\sin(\alpha''-\Theta')}.$$

and introducing the analogous symbols

$$[\mathrm{R}'\,\mathrm{R}'']=\mathrm{R}'\,\mathrm{R}''\sin(\Theta''-\Theta')$$
$$[\mathrm{R}\,\mathrm{R}'']=\mathrm{R}\,\mathrm{R}''\sin(\Theta''-\Theta)$$
$$[\mathrm{R}\,\mathrm{R}']=\mathrm{R}\,\mathrm{R}'\sin(\Theta'-\Theta),$$

the second term of the right hand side of the equation may be written

$$+\left(\frac{[r'r'']}{[r\,r']}-\frac{[\mathrm{R}'\,\mathrm{R}'']}{[\mathrm{R}\,\mathrm{R}']}\right)\cdot\frac{\mathrm{R}\tan\delta'\sin(\Theta'-\Theta)}{\tan\delta''\sin(\alpha'-\Theta')-\tan\delta'\sin(\alpha''-\Theta')}.$$

We have, therefore, assuming

$$\mathrm{M}'=\frac{\tan\delta'\sin(\alpha-\Theta')-\tan\delta\sin(\alpha'-\Theta')}{\tan\delta''\sin(\alpha'-\Theta')-\tan\delta'\sin(\alpha''-\Theta')},$$

$$\text{(14)}\qquad \mathrm{M}''=\frac{\tan\delta'\sin(\Theta'-\Theta)}{\tan\delta''\sin(\alpha'-\Theta')-\tan\delta'\sin(\alpha''-\Theta')},$$

$$\rho''=\frac{[r'r'']}{[r\,r']}\cdot\mathrm{M}'\rho+\left(\frac{[r'r'']}{[r\,r']}-\frac{[\mathrm{R}'\,\mathrm{R}'']}{[\mathrm{R}\,\mathrm{R}']}\right)\mathrm{M}''\,\mathrm{R},$$

an expression as yet perfectly rigorous.

The areas of the triangles $[r\,r']$, &c., are in themselves, when the intervals of time are small, little different from the sectors of the parabola to which they belong, and as little will the ratio of two adjoining sectors be much different from that of the two areas of the corresponding triangles, as at any rate each is smaller than its corresponding sector. For the orbit of the earth this will be equally the case, and the more so as it approximates so nearly to a circle, and as there is consequently a case in which the equality of these ratios may be almost considered as rigorously true, viz. when the intervals are equal. But the sectors are proportional to the times. We may therefore suppose as very nearly true,

$$\text{(15)}\qquad \frac{[r'r'']}{[r\,r']}=\frac{[\mathrm{R}'\,\mathrm{R}'']}{[\mathrm{R}\,\mathrm{R}']}=\frac{t''-t'}{t'-t}.$$

By these equations the last term in (14) becomes quite evanescent, and we obtain

$$(16)\quad M = \frac{t'' - t'}{t' - t} \cdot \frac{\tan\delta' \sin(\alpha - \Theta') - \tan\delta \sin(\alpha' - \Theta')}{\tan\delta'' \sin(\alpha' - \Theta') - \tan\delta' \sin(\alpha'' - \Theta')}$$

$$\rho'' = M\rho,$$

and have consequently an expression for $\frac{\rho''}{\rho}$ in given quantities only.

The formula (15) contains the only approximate suppositions made in Olbers's method. Their admissibility and the cases of exception will be investigated afterwards.

It is now required to find, by means of the equations (16), convenient expressions for r, r'', and the corresponding chord k, which shall contain no other variable but ρ. For the first two there is no difficulty, as

$$r^2 = x^2 + y^2 + z^2$$
$$r''^2 = x''^2 + y''^2 + z''^2.$$

The expression for k, for which we have

$$k^2 = (x'' - x)^2 + (y'' - y)^2 + (z'' - z)^2,$$

is more easily found by a simple geometrical construction which we shall again apply hereafter. Let the place of the sun in space be denoted by S, the two places of the earth at the first and third observations by E and E$''$, those of the comet by C and C$''$. Let a line be drawn though E$''$ parallel to, on the same side with, and equally long as, E C. Calling the termination of the line thus drawn N, and designating its heliocentric coordinates by $x_{\prime}$, $y_{\prime}$, $z_{\prime}$, we have, by what precedes and by the construction,

$$(17)\quad \begin{aligned} x &= \rho\cos\alpha - R\cos\Theta \\ y &= \rho\sin\alpha - R\sin\Theta \\ z &= \rho\tan\delta \\ x'' &= M\rho\cos\alpha'' - R''\cos\Theta'' \\ y'' &= M\rho\sin\alpha'' - R''\sin\Theta'' \\ z'' &= M\rho\tan\delta''. \\ x_{\prime} &= \rho\cos\alpha - R''\cos\Theta'' \\ y_{\prime} &= \rho\sin\alpha - R''\sin\Theta'' \\ z_{\prime} &= \rho\tan\delta. \end{aligned}$$

The two first of these three systems give, by adding the squares,

$$(18)\quad \begin{aligned} r^2 &= \rho^2\sec\delta^2 - 2\rho R\cos(\alpha - \Theta) + R^2 \\ r''^2 &= M^2\varrho^2\sec\delta''^2 - 2M\rho R''\cos(\alpha'' - \Theta'') + R''^2. \end{aligned}$$

Combining the first system with the third, and assuming

$$(19)\quad \begin{aligned} x - x_{\prime} &= R''\cos\Theta'' - R\cos\Theta = g\cos G \\ y - y_{\prime} &= R''\sin\Theta'' - R\sin\Theta = g\sin G, \end{aligned}$$

it is clear that g expresses the chord of the earth's orbit between E and E″, and G the longitude of the first place of the earth when seen from the third. Combining in the same way the second system with the third, and assuming

$$(20)\quad \begin{aligned} x'' - x_{,} &= \mathrm{M}\rho\cos\alpha'' - \rho\cos\alpha = \rho h\cos\zeta\cos\mathrm{H} \\ y'' - y_{,} &= \mathrm{M}\rho\sin\alpha'' - \rho\sin\alpha = \rho h\cos\zeta\sin\mathrm{H} \\ z'' - z_{,} &= \mathrm{M}\rho\tan\delta'' - \rho\tan\delta = \rho h\sin\zeta, \end{aligned}$$

ρh will be the distance of the point N from C″, and ζ and H are the latitude and longitude of C″ when seen from N. As ρ disappears in the equations, h, ζ, and H are known quantities.

The systems (19) and (20) combined together give,

$$x'' - x = x'' - x_{,} - (x - x_{,}), \text{ \&c.}$$

$$(21)\quad k^2 = \rho^2 h^2 - 2g\rho h\cos\zeta\cos(\mathrm{G} - \mathrm{H}) + g^2.$$

The formulæ (18) and (21) assume a form more convenient for logarithmic calculation by representing them as sums of two squares.

With this view let

$$(22)\quad \begin{aligned} \cos\delta\cos(\alpha - \odot) &= \cos\psi \\ \cos\delta''\cos(\alpha'' - \odot'') &= \cos\psi'' \\ \cos\zeta\cos(\mathrm{G} - \mathrm{H}) &= \cos\phi, \end{aligned}$$

and we have

$$(23)\quad \begin{aligned} r &= \sqrt{\left((\rho\sec\delta - \mathrm{R}\cos\psi)^2 + \mathrm{R}^2\sin\psi^2\right)} \\ r'' &= \sqrt{\left((\mathrm{M}\rho\sec\delta'' - \mathrm{R}''\cos\psi'')^2 + \mathrm{R}''^2\sin\psi''^2\right)} \\ k &= \sqrt{\left((\rho h - g\cos\phi)^2 + g^2\sin\phi^2\right)}. \end{aligned}$$

Here ψ and ψ'' are the angular distances of the comet from the sun, and ϕ is the angle at N in the triangle N C C″. Lastly, the calculation may be still more simplified by introducing a new variable quantity. If we suppose

$$\rho h - g\cos\phi = u, \quad \text{or}$$

$$(24)\quad \rho = \frac{u + g\cos\phi}{h},$$

and substitute this value in the first two equations of (23), every quantity will be expressed by u. As $g\sin\phi$ is the perpendicular line drawn in the triangle N C C″ from C to the side N C″, so u is the distance from C″ of the point of intersection of this perpendicular line with the line N C″.

The formulæ here given, which, it appears, have the form most convenient for calculation, have been published by Gauss on the occasion of the second comet of 1813*. In the calcu-

* Zach's [German] Monthly Correspondence, vol. xxviii. p. 504.

lation of g, G, h, H, and ζ, he has adopted a modification which is easily found, and which, (as all formulæ in the whole paper,) supposes the use of his logarithmic tables, by which, from the logarithms of two quantities, the logarithm of their sum or of their difference is immediately found. Indeed, these tables facilitate many calculations so much that every practical calculator should not spare the trouble of getting expert in using them. I will now place together all the formulæ exactly in the symbols used by Gauss, (with the sole exception that the geocentric latitudes are by me denoted by δ,) as all alteration would be for the worse.

The data of the observations, viz. the three α, δ, Θ, R, and t, being given, we have to calculate successively, agreeably to formulæ (16), (19), (20), (22), (23), (24),

$$
(\mathrm{I})\left\{\begin{array}{ll}
\mathrm{M} = \dfrac{t''-t'}{t'-t} \cdot \dfrac{\tan\delta' \sin(\alpha-\Theta') - \tan\delta\sin(\alpha'-\Theta')}{\tan\delta''\sin(\alpha'-\Theta') - \tan\delta'\sin(\alpha''-\Theta')} & \\
\mathrm{R}''\cos(\Theta''-\Theta) - \mathrm{R} & = g\cos(\mathrm{G}-\Theta) \\
\mathrm{R}''\sin(\Theta''-\Theta) & = g\sin(\mathrm{G}-\Theta) \\
\mathrm{M} - \cos(\alpha''-\alpha) & = h\cos\zeta\cos(\mathrm{H}-\alpha'') \\
\sin(\alpha''-\alpha) & = h\cos\zeta\sin(\mathrm{H}-\alpha'') \\
\mathrm{M}\tan\delta'' - \tan\delta & = h\sin\zeta \\
\cos\zeta\cos(\mathrm{G}-\mathrm{H}) & = \cos\phi \\
\cos\delta\cos(\alpha-\Theta) & = \cos\psi \\
\cos\delta''\cos(\alpha''-\Theta'') & = \cos\psi'' \\
g\sin\phi & = \mathrm{A} \\
\mathrm{R}\sin\psi & = \mathrm{B} \\
\mathrm{R}''\sin\psi'' & = \mathrm{B}'' \\
h\cos\delta & = b \\
\dfrac{h\cos\delta''}{\mathrm{M}} & = b'' \\
g\cos\phi - b\,\mathrm{R}\cos\psi & = c \\
g\cos\phi - b''\,\mathrm{R}''\cos\psi'' & = c'',
\end{array}\right.
$$

then

$$
(\mathrm{II})\left\{\begin{array}{l}
r = \sqrt{\left(\left(\dfrac{u+c}{b}\right)^2 + \mathrm{B\,B}\right)} \\
r'' = \sqrt{\left(\left(\dfrac{u+c''}{b''}\right)^2 + \mathrm{B''\,B''}\right)} \\
k = \sqrt{(u\,u + \mathrm{A\,A})} \\
\text{and the value of } u \text{ must now be so determined that} \\
(r+r''+k)^{\frac{3}{2}} - (r+r''-k)^{\frac{3}{2}} = \dfrac{t''-t}{m}
\end{array}\right.
$$

where

$$m = \frac{1}{6\,K}, \quad \log m = 0{\cdot}9862673,$$

If we make use of the table given at the end, u must be so determined that if

$$\eta = \frac{2\,K}{(r + r'')^{\frac{3}{2}}}\,(t'' - t),$$

then

$$k = \frac{2\,K}{(r + r'')^{\frac{1}{2}}}\,(t'' - t)\,\mu,$$

where $\log 2\,K = 8{\cdot}5366114$.

In making these trials hitherto, a value of u was assumed and from it r, r'', k were computed. The substitution of the values thus found in Lambert's formula showed whether the assumed value of u was the true one. If not, it was varied until the formula was complied with. But this way of proceeding has this disadvantage, that in general u, according to its geometrical signification, does not admit of an approximate valuation, and one might probably assume at first a value very far from the truth. The process proposed by Olbers in the preceding paper appears to be much preferable. From practical, and, as far as here applicable, from theoretical reasons, it will be perceived that $r + r''$ will be seldom or never $\angle$ 1, and likewise seldom $\urcorner$ 3. The value $r + r'' = 2$ will, at least in most cases, not be far from the truth. If, therefore, by means of the table, from $r + r'' = 2$, the corresponding k is determined, and from this value of k the u, and thence r and r'', the comparison of the new value of $r + r''$ with the one assumed for this sum, will prove how far the assumption was correct. The new value may now be again applied, and we may thus continue until a perfect agreement is obtained.

The formulæ being so simple, it is interesting more closely to investigate the steps of this process. The factor μ may be considered as a constant quantity, it being at any rate, in all cases which can occur, little different from unity. Making, for brevity, $r + r'' = s$, the differential equations of the formulæ, as they are successively applied, will give the following relations:

$$d\,k = -\tfrac{1}{2}\,k\,\frac{d\,s}{s}$$

$$d\,u = \frac{k}{u}\,d\,k$$

$$d\varrho = \frac{1}{h}\, du \qquad d\varrho'' = \frac{M}{h}\, du$$
$$dr = \sec\delta \cos C\, d\rho$$
$$dr'' = \sec\delta'' \cos C''\, d\varrho''.$$

If we designate the value of s originally assumed by s_0, its correction by $d s_0$, so that

$$s_0 + d s_0 = s,$$

or the true value again by s_1, the value of s, resulting from the assumed value s_0, and its correction by $d s_1$, so that in like manner $s_1 + d s_1 = s =$ the true value, we have

$$d s_1 = -\left\{\frac{\rho \sec\delta \cos C + \rho'' \sec\delta'' \cos C''}{2(r + r'')}\right\} \cdot \frac{k^2}{\rho h . u} \cdot d s_0,$$

in which formula the angles C and C″ are those angles which represent the angular distance of the earth from the sun when seen from the comet, or the angles C and C″, in the triangles S C E and S C″ E″. The formula supposes that the assumed value s_0 is not so far distant from the truth as that the differential for s may not be applied with some approximation; it is likewise clear, that by $d s_0$, $d s_1$, may be understood not only the corrections of the numerical values but likewise the corrections of their logarithms.

On account of the circumstance just mentioned, we may simplify the formula by putting for the sum of the terms $\varrho \sec\delta \cos C$ and $\varrho'' \sec\delta'' \cos C''$, this one, $2\,\varrho' \sec\delta' \cos C'$, by which, at least in reference to the general form, nothing is changed. We may besides designate for the sake of simplicity $\rho' \sec\delta'$, or the true distance, by the single letter Δ'. Besides, in order to have such quantities only as have a geometrical meaning, let us consider the triangle N C C″ above referred to. The sides of this triangle are k, ϱh, g, and the angle opposite to k is ϕ. Let the angles opposite to ρh and g be denoted by χ and χ''; we have

$$\varrho h . u = k \cos\chi'' . \rho h$$
$$= k (k - g \cos\chi).$$

By this means we obtain this simple form, if

$$q = \frac{\Delta' \cos C'}{(r + r'')} \cdot \frac{k}{k - g\cos\chi}$$
$$d s_1 = - q . d s_0.$$

The factor q can only become negative when either cos C becomes negative, or $g \cos\chi > k$. The first case supposes that the comet is within a sphere, the centre of which is the sun, and the radius the distance of the earth from the sun, so that it would be very near both to the sun and to the earth. The second case, on the contrary, requires that $g > k$, or as

the linear velocity of the comet at the time of the second observation is $= \sqrt{\left(\frac{2}{r}\right)}$ the linear velocity of the earth being assumed $= 1$, while the ellipticity of its orbit is neglected, so that approximately

$$1 > \frac{2}{r'}, \quad r' > 2,$$

consequently $s > 4$. Both cases, comets very near to the earth and the sun, and comets very distant from the sun, are rare, especially the latter. Commonly q will have a positive value.

Hence it follows that employing first s_0, then the following value s_1, next the one resulting from that one s_2, and so on, we shall obtain alternately values too small and too great, an advantage of no small consequence for the rapid approximation. For the corrections, or that which is to be added to the assumed value in order to obtain the true one, will be

$$\begin{aligned} d\,s_1 &= -\,q\,d\,s_0 \\ d\,s_2 &= -\,q\,d\,s_1 = +\,q^2\,d\,s_0 \\ d\,s_3 &= -\,q\,d\,s_2 = -\,q^3\,d\,s_0, \text{ \&c.} \end{aligned}$$

We find indeed, in the four examples calculated by Olbers, this change of signs occurring three different times. Only in the case of the first comet of 1805, where the case above referred to occurred, the successive values are $s_0 = 2$, $s_1 = 1{\cdot}413$, $s_2 = 1{\cdot}328$, $s_3 = 1{\cdot}318$, which are all in *excess*.

By means of the same consideration we may, when three values have been calculated, approximate to the truth by an easy interpolation. We will suppose s_1, s_2, s_3 any three successive values of s, each of which has been derived from the one next preceding it. Let us find the arithmetical differences:

$$\begin{array}{ccc} s_1 & & \\ & \Delta\,s_1 & \\ s_2 & & \Delta^2\,s_2 \\ & \Delta\,s_2 & \\ s_3 & & \end{array}$$

we shall have approximately

$$\begin{aligned} \Delta\,s_1 &= \quad (1+q)\,d\,s_1 \\ \Delta\,s_2 &= -\,(1+q)\,q\,.\,d\,s_1 \\ \Delta^2 s_2 &= -\,(1+q)^2\,d\,s_1, \end{aligned}$$

consequently,

$$d\,s_1 = -\frac{(\Delta\,s_1)^2}{\Delta^2\,s_2}$$

$$d\,s_3 = -\frac{(\Delta\,s_2)^2}{\Delta^2\,s_2},$$

and generally an approximate value of q,

$$q = -\frac{\Delta s_2}{\Delta s_1},$$

which, if necessary, might be employed in an insulated trial. We may employ $d\,s_1$ or $d\,s_3$ according as the one or the other is smaller.

The latter selection and in general the more or less rapid approximation of the trials depend on the value of q. The first factor,

$$\frac{\Delta' \cos C'}{r + r''},$$

will in almost all cases be a very small fraction. The magnitude of the second factor will, on the contrary, mainly depend on the value of χ. Agreeably to the construction, χ is the angle formed by the chords of the orbits of the earth and the comet, if these chords are so placed together that the first place of the earth coincides with the first place of the comet, and the directions are made to correspond to the directions of both bodies. When the motions are parallel, χ will consequently be $= 0$, and $\cos \chi$ will have its greatest positive value; but when the motions are in contrary directions, $\chi = 180°$, and $\cos \chi$ has its negative maximum. Hence it follows that likewise this factor will in most cases, at the discovery of a comet, be a real fraction, or will at least not differ much from unity, because by the nature of the case comets will always be first seen when their course is rather opposed to that of the earth, and directed towards the earth, than when both move in the same direction in parallel courses. In the latter case they might have been seen before. The ambiguity which seems to exist in this manner of instituting the trials, viz. whether, on account of $u^2 = k^2 - A^2$, u is to be taken positive or negative, disappears in practice almost altogether. By the geometrical meaning of u it is clear that it can only become negative when the angle χ'' is obtuse, consequently $g > k$, and, after what precedes, $s > 4$. Cases of this kind are of rare occurrence, and may, when they occur, be easily decided.

It appears, therefore, that this way of instituting the trials has greatly the advantage over the usual process, partly on account of the value immediately to be applied of $s_0 = 2$, partly on account of the methodical manner in which the successive assumptions result from the first without any arbitrary suppositions. Only when u is very small, which quantity for that very reason will then be derived with less accuracy from the combination of k and A, the proceeding may perhaps be more circuitous. The remarks of Dr. Olbers on cases where, in place of $s_0 = 2$ immediately, a smaller or larger value may be applied,

are, of course, likewise here applicable. The smallest valueof s is, agreeably to what is said above, $B + B''$; when this value is itself nearly $= 2$, in the cases of comets which are near the quadrature, or much smaller than 2, for comets which are nearer to the conjunction or opposition, a greater or less value may be chosen in the beginning, especially with a due regard to the values of b and b'' compared to those of c and c''.

In order to give an application of these formulæ I will give here the calculation of the second comet of 1813, which has become a model by Gauss's paper, and I will apply it to the method of trials here explained.

The observations of Göttinger of the 7th, 14th, and 21st of April, 1813, gave for this comet the following data:

$$t = 7{\cdot}55002$$
$$t' = 14{\cdot}54694$$
$$t'' = 21{\cdot}59931.$$

$$\alpha = 271°\ 16'\ 38'' \qquad \delta = +29°\ \ 2'\ \ 0''$$
$$\alpha' = 266\ \ 27\ \ 22 \qquad \delta' = +22\ \ 52\ \ 18$$
$$\alpha'' = 256\ \ 48\ \ \ 8 \qquad \delta'' = +\ 9\ \ 53\ \ 12$$
$$\odot = 17\ \ 47\ \ 41 \qquad \log R = 0{\cdot}00091$$
$$\odot' = 24\ \ 38\ \ 45 \qquad \log R' = 0{\cdot}00175$$
$$\odot'' = 31\ \ 31\ \ 25 \qquad \log R'' = 0{\cdot}00260.$$

Hence will be found:

$$\log M = 9{\cdot}75799$$
$$G = 113°\ 43'\ 57''$$
$$\log g = 9{\cdot}38029$$
$$H = 109°\ \ 5'\ 49''$$
$$\zeta = 44\ \ 13\ \ 9$$

$$\log h = 9{\cdot}81477$$
$$\log A = 9{\cdot}22527$$
$$\log B = 9{\cdot}98706$$
$$\log B'' = 9{\cdot}86038$$
$$\log b = 9{\cdot}75645$$
$$\log b'' = 0{\cdot}05028$$
$$c = +0{\cdot}31365$$
$$c'' = +0{\cdot}95443.$$

As a first trial (although it may be easily seen that s must be considerably greater than 2) I assume

$$s_0 = 2, \quad \log s_0 = 0{\cdot}30103.$$

We have next, since

$$t'' - t = 14{\cdot}04929$$
$$\log 2\,K\,(t'' - t) = 9{\cdot}68427,$$

whence the calculation stands thus:

$$\begin{array}{ll} & 9{\cdot}68427 \\ & \underline{0{\cdot}15051} \\ & 9{\cdot}53376 \ldots \tan\eta = 9{\cdot}23273 \quad \eta = 0{\cdot}1709 \\ \log\mu \ldots & \underline{0{\cdot}00053} \\ \log k \ldots & 9{\cdot}53429. \end{array}$$

$$\begin{array}{lll} \log A \ldots 9{\cdot}22527 & \log(u+c) \,.\, 9{\cdot}78660 & \log u+c'' \,.\, 0{\cdot}09780 \\ \log k \ \ldots \underline{9{\cdot}53429} & \log b \ldots \ldots \underline{9{\cdot}75645} & \log b'' \ldots \underline{0{\cdot}05028} \\ \qquad\quad \underline{9{\cdot}94013} & \qquad\qquad 0{\cdot}03015 & \qquad\qquad 0{\cdot}04752 \\ \log u \ldots \ 9{\cdot}47442 & \log B \ldots \ldots 9{\cdot}98706 & \log B'' \ldots 9{\cdot}86038 \\ \quad u = +\,0{\cdot}29814 & \qquad\qquad \underline{9{\cdot}86996} & \qquad\qquad \underline{9{\cdot}92349} \\ u+c = +\,0{\cdot}61179 & \log r \ldots \ldots 0{\cdot}16019 & \log r'' \ldots 0{\cdot}12403 \\ u+c'' = +\,1{\cdot}25257. & & \end{array}$$

$$\begin{array}{r} \underline{0{\cdot}28332} \\ \log s_1 = 0{\cdot}44351. \end{array}$$

Continuing this value we obtain

$$\begin{aligned} \log r &= 0{\cdot}13612 \\ \log r'' &= 0{\cdot}10890 \\ \log s_2 &= 0{\cdot}42375 \end{aligned}$$

and by means of this latter again,

$$\begin{aligned} \log r &= 0{\cdot}13933 \\ \log r'' &= 0{\cdot}11092 \\ \log s_3 &= 0{\cdot}42639. \end{aligned}$$

Forming now the differences thus,

$$\begin{array}{lcc} \log s_1 = 0{\cdot}44351 & & \\ & -1976 & \\ \log s_2 = 0{\cdot}42375 & & +\,2240 \\ & +\ 264 & \\ \log s_3 = 0{\cdot}42639 & & \end{array}$$

we obtain

$$d \log s_3 = -\frac{(264)^2}{2240} = -\,31,$$

or, $$\log s = 0{\cdot}42608,$$

and by interpolation,

$$\begin{aligned} \log r &= 0{\cdot}13895 \\ \log r'' &= 0{\cdot}11068: \end{aligned}$$

values which are strictly true, as a repetition of the calculation would prove. Gauss finds 0·13896 and 0·11068. One would in this example have come near the truth by two trials only, for from

$$\begin{array}{lcc} \log s_0 = 0{\cdot}30103 & & \\ & +\,14248 & \\ \log s_1 = 0{\cdot}44351 & & -\,16224 \\ & -\ \ 1976 & \\ \log s_2 = 0{\cdot}42375 & & \end{array}$$

we get

$$d \log s_2 = +\frac{(1976)^2}{16224} = +\,241,$$

or $$\log s = 0{\cdot}42616.$$

A calculation made with this value would have given the required value with perfect accuracy.

As soon as s and consequently u, ρ and ρ'' are found, the elements of the parabola may be derived in various ways from the two extreme observations. It will, however, be interesting previously to investigate the degree of approximation obtained by the assumption in (15) and the applicability of the formulæ (16).

[To be continued.]

III. *On the Summation of slowly converging and diverging Infinite Series.* *By* J. R. Young, *Professor of Mathematics in Belfast College.*

[Continued from vol. vi. p. 354.]

THE foregoing examples have been chosen of the form

$$S = b - cx + dx^2 - ex^3 + \&c.$$

because it is only for slow series of this form that the transformation furnished by the differential theorem offers any practical facilities. When the series is of the form

$$b + cx + dx^2 + ex + \&c. \quad \dots \quad \dots \quad (D.)$$

we may indeed convert it into

$$\frac{1}{1-x}\left\{ b - \frac{\Delta x}{1-x} + \frac{\Delta^2 x^2}{(1-x)^2} - \frac{\Delta^3 x^3}{(1-x)^3} + \&c. \right\} \quad \dots \ (E.)$$

by simply substituting $-x$ for x, in the formula (B.), and this is a form to which the foregoing arithmetical process may be applied. But it is easy to see that that process would conduct us to the original series, and would terminate in the actual summation of its successive terms; for by the formula (B.) it appears that the new series, into which (E.) would be transformed by the process alluded to, would proceed according to the powers of $\frac{x}{1-x} \div \left(\frac{x}{1-x} + 1\right)$, instead of according to the powers of $\frac{x}{1-x}$, as at present, that is, it would proceed according to the powers of x; and, agreeably to this change, the factor $\frac{1}{1-x}$, which multiplies the series (E.), would become simply 1; so that (E.) would thus be converted into a series of the form

$$b + c'x + d'x^2 + e'x^3 + \&c.$$

which being equal to (D.), independently of particular values of x, must be identical with it; hence the transformation (E.) does not assist us in the summation of (D.). Slow series, however, of the form (D.) may, when b, c, d, &c. are a series of divisors in arithmetical progression, be easily changed into others, of quicker convergency, by a method which a single example will fully illustrate. Suppose, for instance, the summation of $x + \frac{x^3}{3} + \frac{x^5}{5} + \frac{x^7}{7} + \&c.$ be required. Put

$$x + \frac{x^3}{3} + \frac{x^5}{5} + \frac{x^7}{7} + \&c. = S_1$$

$$\because \frac{x}{3} + \frac{x^3}{5} + \frac{x^5}{7} + \frac{x^7}{9} + \&c. = \frac{S_1 - x}{x^2}.$$

Subtracting

$$\frac{x}{1.3} + \frac{x^3}{3.5} + \frac{x^5}{5.7} + \frac{x^7}{7.9} + \&c. = \frac{1}{2}\left\{S_1 - \frac{S_1 - x}{x^2}\right\} = S_3.$$

Hence

$$S_1 = \frac{2\,x^2\,S_3 - x}{x^2 - 1} = \frac{x}{x^2-1}\left\{2\,x\,S_3 - 1\right\},$$

and consequently S_1 will be obtained by summing the more rapid series S_3. Again,

$$\frac{x}{1\,.\,3} + \frac{x^3}{3\,.\,5} + \frac{x}{5\,.\,7} + \frac{x^7}{7\,.\,9} + \&c. = S_3$$

$$\therefore \frac{x}{3\,.\,5} + \frac{x^3}{5\,.\,7} + \frac{x^5}{7 \cdot 9} + \frac{x^7}{9\,.\,11} + \&c. = \frac{S_3 - \frac{x}{1.3}}{x^3}.$$

Subtracting

$$\frac{x}{1.3.5} + \frac{x^3}{3.5.7} + \frac{x^5}{5.7.9} + \frac{x^7}{7.9.11} + \&c. = \frac{1}{4}\left\{S_3 - \frac{S_3 - \frac{x}{1.3}}{x^2}\right\} = S_5.$$

Hence $$S_3 = \frac{4\,x^2\,S_5 - \frac{x}{1.3}}{x^2 - 1} = \frac{x}{x^2-1}\left\{4\,x\,S_5 - \frac{1}{1\,.\,3}\right\};$$

and thus S_3, and therefore S_1, may be obtained by summing the more converging series S_5. And generally

$$S_{n-2} = \frac{x}{x^2-1}\left\{(n-1)\,x\,S_n - \frac{1}{1.3\ldots n-2}\right\};$$

so that by summing a few terms of S_n we may, by this formula, obtain near approximate values to S_{n-2}, S_{n-4}, &c. in succession, and finally, to S_1, each step being deduced from the preceding by simple arithmetical operations.

IV. *On the Importance of Studying and Preserving the Languages spoken by Uncivilized Nations, with the view of elucidating the Physical History of Man. By* Dr. HODGKIN.*

BEFORE proceeding to the objects which it is my design to lay before you, I would beg leave to take a rapid glance at the range which our Association may be considered to embrace. The study of language may, I conceive, be taken up on several distinct grounds: 1st. It may be considered metaphysically, by which I mean the consideration of language generally, as the means of giving expression to the feelings and operations of the mind. To this head might belong the question whether language is coeval with the creation of man, or has been gradually worked out by the development of his faculties, having nothing more than the mere capability of utterance as the original material with which to work. I confess that whilst it is impossible to doubt the progressive development of language, I am inclined to unite with those who admit the former proposition. The metaphysical philologist may nevertheless pursue his speculations and inquire hypothetically into the mode in which language might be progressively built up to satisfy the wants of man. With this inquiry, however, I have little inclination to meddle, but I would take this occasion to mention an interesting essay in some degree bearing on this subject, recently written by Benjamin Harrison, jun., of Christ Church, Oxford. To this same division of the subject belong the general or universal principles of grammar. 2ndly. Another division of the subject, to which there is some difficulty in giving a name, forms a most essential part of what are called the Belles Lettres. It has not so much to do with the wide range offered by the numerous languages spoken in different parts of the globe as with the profound and critical knowledge of a few to which common consent has given a prominent position. These are studied in all the varieties of style and idiom as well as in the modifications which at different periods they have exhibited. This branch of the subject necessarily requires an intimate and minute acquaintance with the best authors who have written in these languages; and the individual who has successfully devoted himself to this branch of our subject, is considered *par excellence* a learned man and a great scholar. It is not my object to detract from the estimation which pure and elegant Latinity and an intimate acquaintance with the dialects, idioms, and metres of the Greeks have by common consent

* Read before the Philological Society by Dr. Hodgkin, in the course of the present session, and now printed with the permission of that Society, in order to make known some of the suggestions contained in it.

conferred; but it is not in my power to engage your attention in this department. There is another, the 3rd, which I shall mention, which seems more associated with the studies of my own profession; this may be styled the physiological department of philology. To this department belong the interesting researches respecting the production of the vowel sounds, the modification of the voice by the movements of the larynx and the cooperation of the tongue, teeth, lips, and nose. It is not, however, in this department that I propose seeking this evening's occupation, yet I would remark in passing that there are certain modifications of language which it seems necessary to refer to this department, and which cannot fail to introduce a difficult complication, and even serious errors, if they are allowed to be blended with the points of inquiry which legitimately belong to the next division which I shall have to notice. I allude to certain modifications and transitions of sounds which probably pervade all languages, as the result of physiological or organic causes, rather than as proceeding from their mutual relationship as branches of a particular stock. Although it is not my intention to enter minutely into this question, I shall offer a few examples by way of illustration: the substitution of one letter for another is a modification of the kind to which I am alluding. Thus the B is changed into a V in converting the Latin Diabolus into the Italian Diavolo. A similar change seems to have taken place with the modern Greeks, who say *vivlos* for *biblos* (βιβλος), and *vasilefs* for *basileus* (βασιλευς). It may, however, be questioned whether this is not the true ancient pronunciation preserved by the descendants of the Greeks, from which other nations have seceded, yet I am aware that there are strong reasons for doubting that this is the case. That this change is not a characteristic of a particular stock in language, but rather one of those changes which may be common to them all, and be referrible to some physical cause, may be inferred from the fact that a similar substitution is to be found on the north coast of Africa amongst persons who speak Arabic*. I observed this in a gentleman from Morocco, and I found that his substitution of B for V,—which I think he made reciprocally, so that he would have said 'Biridi certat vacca benafro,'—was connected with a deficiency in the perception of the sounds of these two letters. He could distinguish no difference between Bacca and Vacca. The substitution of V for W and W for V, which is so characteristic of the vulgar London pronunciation, seems to be

* I have noticed several instances of the similar substitution of one letter for another in the vocabularies of different dialects of the Polynesian language.

of precisely the same character. An individual who exhibited this peculiarity asserted that his ear could detect no difference between Weal and Veal, Winegar and Vinegar. Whether this insensibility be general amongst those who have acquired this vulgar London peculiarity I am not prepared to say, but I have known a similar insensibility to exist where another substitution of the same kind had taken place. In some parts of Worcestershire and Herefordshire a striking error prevails in the use of the aspirate; we hear it employed where it should be suppressed, and it is suppressed where it should be employed. Although this is done so systematically that it seems like the result of design and principle, yet, as I have just stated, I know that it may take place from a want of perception of the difference existing between the two sounds *. I heard a gentleman from that district say that he could not distinguish *as* from *has*. I am unable to say how far changes of this kind are to be attributed to a defective appreciation of the sounds produced, but I believe there can be little doubt that such changes are rather to be attributed to a want of aptitude in the organ producing than in the organ perceiving the sound. A cause of this latter kind operates to prevent many persons from pronouncing the TH, to which, in consequence, they give the power of T or of D. A similar difficulty occasions the L to be pronounced like R, as, for example, "the *Rong grories* of majestic Rome," for the *Long glories*, &c. Children, in learning to speak, often, transiently, exhibit difficulties of this kind, which they for the most part speedily surmount, unless the difficulty be very great, in which case it may become permanent.

The commutation of one letter for another, as is exhibited in the declinable parts of speech, in the composition of compound words, and also in the concurrence of words in a sentence, of which there are so many striking examples in the Greek language, are doubtless to be referred to some physical cause which may sometimes be found in the greater facility of utterance, and at others in the more agreeable sound conveyed to the ear. The manner of contraction from double vowels to diphthongs, and from long vowels to short, is to be referred to a similar cause, as being rather dependent on a physical cause than to be regarded as a peculiarity connected with a particular language. The contraction of OI into I in Greek seems to be quite analogous to the vulgar pronuncia-

* [An instance illustrative of this is within our own knowledge, in which a person who was originally incapable of distinguishing between aspirated and unaspirated sounds, and who therefore uttered them indiscriminately, has gradually acquired both the perception and the right utterance of them.—Edit.]

tion of *pint* and *pison* for *point* and *poison.* It is by no means improbable that some of these peculiarities of pronunciation and the predominance or suppression of certain sounds in particular districts, although proceeding from physical causes, and therefore to be regarded as distinct from those characters which indicate connexion with a particular stock, may nevertheless be characteristic of a particular race, the organic peculiarity prevailing, like those of colour, form of the head, expression of countenance, stature, and the like, from hereditary transmission. That peculiar dialect of English spoken by negroes when living as an enslaved or otherwise oppressed race amongst the English or their descendants, though partly to be ascribed to the neglect of their education and to the encouragement which whites give to this dialect, by themselves falling into it when conversing with negroes, is doubtless principally to be ascribed to the same physical causes which give a characteristic softness to several of the languages on the west coast of Africa, and thus, like the colour of their skin and the form of their features, indicates the stock from whence they are derived, independently of any infusion of words from their own languages with which they may have corrupted the English. The investigation and classification of these changes, influenced by physical causes, might greatly facilitate the labour of those who may apply themselves to the acquisition of several languages, as well as aid those investigations which more particularly fall under the class of which I am next to speak.

The fourth and last division of the subject may perhaps be called the Natural History of Language, its object being to investigate and classify the numerous languages which are spoken upon the face of the globe; to refer them, as far as they admit of being so traced, to different primitive stocks or languages, from which there is reason to apprehend that many of them have proceeded as from a parent or common stock; and to discover, as far as inherent and collateral evidence can render possible, the modifications which the intermixture of language derived from a different stock may have produced, and the time and mode by which these changes by infusion have been brought about. These inquiries are perhaps the most important which can be undertaken for the elucidation of the physical history of man; this division of the subject may therefore be regarded as one of the most interesting and important in which man can be engaged. Although the arduous and successful labours of many distinguished philologists, amongst whom must be especially mentioned Herder, Adelung, Vater, W. and A. Humboldt, Rask, Klaproth, Prichard, Mars-

den, Grotefend, Crawford, &c., have done much to excite an interest in this subject, not only in its elucidation, but by extending the inquiry into new regions, yet it is still very far from having obtained the estimation and encouragement which it merits. If I am not greatly mistaken, the thorough investigation of language, on this extensive scale, is absolutely essential to the philologist and to the antiquary or historian, to render theirs integral sciences. Without it their deepest researches and most successfully rewarded exertions only go to the production of a fragment, which, though mighty and splendid, is but a fragment, failing to convey a just idea of the whole. In this view of the subject, Philology and History may be considered somewhat in the same state as the science of Botany would be were it based solely upon the Flora of a particular district; and some of its cultivators may be compared to the ingenious and successful florist who brings out the varieties and beauties which a few species are capable of producing, rather than to the Linnæuses, the Jussieus, the Decandolles, and the Browns. It may perhaps be more correct to compare the state of that branch of Philology which we are now considering to Geology at the time of Buffon, when many and valuable detached facts had been found out and recorded, when the importance of the conclusions to which they might lead were acknowledged, but when these conclusions could not be drawn, and when that great man and some others trusted to supply the deficiency by systems wrought out by their own vigorous imaginations.

If there be any value to be attached to Philology on the comprehensive scale to which I have alluded, we must have a great accession of numerical strength in the class of patient and able observers who may be content to amass facts, both in language and on collateral subjects, without any ruling bias as to the result to which these observations may seem for a time to tend. This is the course which has been successfully pursued with respect to Geology, and which has made it perhaps the most popular subject of investigation of the present day. There is, however, an important difference between the pursuit of Geology and that of Philology to the extent which I am pointing out. If the philological investigations are the more difficult and laborious, and are further removed from the reach of those who may feel an interest in the pursuit, and if it be on this account a less inviting science, there are reasons which do not exist in the case of Geology, or perhaps in any other science, to urge to the prompt and zealous pursuit of it. The geological facts which escape observation or record in this year, or even in this century, may be investigated with equal or greater success in centuries to come. The same may be said

of almost every other science except that of Philology in its most comprehensive sense. The precious materials with which alone this fabric can be constructed are like the fleeting moments of time itself, which are removing them irrevocably from our research. It becomes therefore a matter of very serious consideration for those who feel the importance of Philology, or rather of that branch of it with which we are now occupied, to bring into its service without delay all the available strength which can be mustered.

I must confess that my object in bringing this paper before my fellow members of the Philological Society has been to submit to their judgement certain measures which I have been led to believe might, with their sanction and support, be made very successfully to conduce to the investigation of this branch of Philology as well as to other researches intimately connected with it.

Before I proceed to offer these suggestions it may not be amiss to take a superficial survey of the present state of the subject, in order that we may have before us, not only the vast extent of the present deficiency, but also some of the grounds of encouragement which prompt to perseverance notwithstanding the apparent discouragement which exists.

I shall adopt as the basis of this sketch the tabular view which is given by my excellent friend Dr. Prichard, which, although it requires alteration in some points, is, I apprehend, the most correct general survey that we at present possess *.

From this table it appears that the Indo-European, the Western Asiatic, the Northern Asiatic and Eastern European, and the Chinese and Indo-Chinese, including those known to have existed as distinct, with those which at present do so, comprise nearly two hundred nations, to which we may ascribe forty-one languages, besides many which are unknown or not mentioned.

According to the same authority we are at present more or less acquainted with upwards of forty different African races, several of which are numerously subdivided. There are above twenty ascertained languages, and of some of these there appear to be various dialects; and it is admitted that there are besides thirty-eight languages wholly unknown to us. Amongst the comparatively recent attempts to increase our knowledge of the African languages, I may mention that my friend Jomard, a member of the French Institute and one of the *Commission*

* [A summary classification of languages purporting to carry still further their natural and ethnographical arrangement, has been given by Mr. Beke, in his *Origines Biblicæ*, ch. x. p. 231—235, and also in his 'Views in Ethnography, &c.' published in Jameson's New Edinburgh Phil. Journ. for April last.—Edit.]

d'Egypte, has paid considerable attention to the Jaloff language and has composed a grammar, and I believe a dictionary of the same. Hannah Kilham, a minister in my own society, whose zeal led her to make three voyages to Africa, reduced as many as five or six African languages, besides the Jaloff, to a written form. A laborious article has been written on the Birbir language. The missionaries in Southern Africa have paid some attention to the languages spoken in that quarter, and the languages of the Copts and Abyssinians have not been neglected.

The widely scattered inhabitants of the islands of the Indian Archipelago and Pacific Ocean compose twenty-four groups, which seem to be referrible to two or three principal divisions. In many instances the languages spoken by these groups are confessedly allied. They are by most admitted to have more or less affinity to the Malay, properly so called. In some instances the languages are unknown, and in others they are said, but upon what authority I know not, to be quite peculiar. The similarity between the languages spoken in several of these groups was noticed by Captain Cook, and has been confirmed by many voyagers since his time. The subject of these languages has been scientifically taken up by Marsden and Crawford, by some German philologists, and by missionaries employed by this country. Articles on this subject have lately appeared in the Quarterly Review, and I have recently been informed that Baron W. Humboldt is at present engaged in a work on these languages*. I must not attempt to enter into any of the views of these authors, but I cannot omit to notice the work and views of another author which appear to me to deserve considerable attention. I allude to the Essay of Dr. Lang on the origin and migration of the Polynesian nation. One of the objects of the Doctor's work is to show that the several dialects spoken in the islands of the Pacific Ocean are branches of the Malay stock; and again, that the Malay language, with its ramifications, is of Asiatic origin, and if not derived from the Chinese, is at least related to it. He meets the objection which might be raised from the fact that the Polynesian and other branches of the Malay stock are not so monosyllabic as the Chinese language, by observing that the former are often lengthened by prefixed or suffixed particles, which seem to be added in order to increase the

* Since this paper was read to the Philological Society, this excellent man and profound philologist has paid the debt of nature. Of the state in which he has left the paper here referred to we can give no information. Baron W. Humboldt's Posthumous Works, including the memoir in question, have recently been announced and preparing for publication.—Edit.

number of vowel sounds. Thus, the word *tong*, which signifies *east* in the Chinese, is converted into *tonga* in the language of New Zealand. He further notices the prevalence of particular sounds, as that of the *ng*, which occurs either at the beginning or the end of words in both languages, and of the particles *e* or *y*, *pa*, *pe*, *te*, *ka*, and *ko*, or *to*, which are of frequent use in them, and, as it would appear, in similar modes and for similar purposes. Besides these points, indicating a similarity in character, there are some in which there is an evident similarity of meaning, which seems to indicate still more strongly than the instances last mentioned the absolute affinity of these languages.

Having endeavoured to show that the Malay language is a kindred tongue to the Chinese, he points out that the language spoken by the Malays properly so called, has received, at two different periods, important additions from distinct languages, in a manner somewhat similar to that in which the Anglo-Saxon, the original of our own tongue, became blended with Norman-French. These admixtures, or infusions as Dr. Lang calls them, consisted, in the case of the Malay language,

1st, Of the introduction of a considerable number of Sanscrit words. This modification of the Malay language appears to have been accompanied by a corresponding change in the religion of the Malays, who are supposed to have adopted that of their Sanscrit teachers. 2nd, The next infusion which the Malay language received was from some of the enterprising followers of Mahomet, who introduced both the language and the religion of the Koran. This introduction of Sanscrit and Arabic words into the Malay language appears to explain in a satisfactory manner some of the differences between the Malay language as it exists at the present day and the dialects which are found in those widely scattered islands, from Madagascar to Easter Island, the inhabitants of which are generally admitted to speak a language related to the Malay. The Polynesian language, consisting of several closely allied dialects, appears to have been derived from the Malay before it had received the Arabic or even the Sanscrit infusion, and consequently points to a very remote period, at which the occupation of those islands commenced.

Dr. Lang endeavours to show that this connexion, as indicated by the state of the language, is confirmed by many features of resemblance between the present or past habits of the Oceanic islanders and the Asiatic race from whence they are supposed to have sprung. Some of the most striking instances of similarity consist in the separation of the inhabitants into castes, keeping themselves punctiliously from each other; in

the adoption of a different language according as the person speaking is of one class and the individual addressed is of another; in their treatment of females; in the superstitious observance of Taboos; in the possession of the rite of circumcision; in some of their games; and in the chewing of the Betel-nut.

The Doctor devotes some pages to account for the origin of a revolting peculiarity which has characterized nearly, if not all, the widely extended ramifications of the Polynesian race, namely, their propensity to eat human flesh. The Doctor accounts for it by supposing that it originated in the urgent calls of extreme hunger experienced by those who made the long and disastrous voyages which have given a kindred population to widely remote islands. In these voyages, often performed without design, under the irresistible influence of wind or current, the stock of provisions must often have been extremely inadequate, and the starving islanders in their canoes may thus have been impelled to partake of the flesh of such of their companions as may have first perished from want, or they may have sacrificed one or more of their number to sustain the rest. The recurrence of such causes, in conjunction with the warlike habits of the people and their human sacrifices, would tend to encourage a practice which the almost total absence of animal food, excepting fish, would be likely still further to promote.

I need not, at present, further pursue the analysis of Dr. Lang's work as respects the Polynesian nations' dialects and manners; but before I notice that part of his work in which he endeavours to connect the Polynesian with the American nations, I must mention, from the before-mentioned tabular view given by Dr. Prichard, what appears to be the state of America with respect to its nations and languages. The former appear considerably to exceed 300, and the latter seem to be proportionably numerous, but of these many are unknown. Notwithstanding that a vast number of these languages are stated to be totally distinct, we have the authority of some excellent philologists, and more particularly that of Baron Alexander de Humboldt, that there is a sort of common genius or constitution pervading all these languages as far as they have been examined, and which unites them into one group, whilst it distinguishes them from nearly or quite all others. A more recent traveller, Dr. Von Martius, of whose interesting memoir on the state of the civil and natural rights of the aborigines of the Brazils a translation is given in the Journal of the Geographical Society, confirms the fact of the very great number of the American languages, and mentions that in Brazil alone more than 150 languages and dialects are spoken,

by more than 250 nations completely broken up and mostly incapable of communicating with each other. The Doctor adduces this statement in conjunction with the present state of the American Indian tribes in support of his hypothesis that the American race is altogether distinct from the rest of mankind, and that the uncivilized tribes with which we are at present acquainted are the debris of one or more nations who, having attained civilization, subsequently relapsed into a state of barbarism, which allowed of their being broken up into distinct nations and tribes, whose migrations during countless ages has brought them into the deplorable state in which they now exist, and which he thus describes:

"In fact, the present and future condition of this red race of men, who wander about in their native land, without house or covering, whom the most benevolent and brotherly love despairs of ever providing with a home, is a monstrous and tragical drama, such as no fiction of the poet ever yet presented to our contemplation. A whole race of men is wasting away before the eyes of its commiserating contemporaries; no power of princes, philosophy, or Christianity can arrest its proudly gloomy progress towards a certain and utter destruction." (Journal of the Royal Geographical Society, vol. ii.)

Though I shall not stop to combat this opinion, I cannot refrain from expressing my confident hope that a better fate awaits that interesting portion of the human family, and that Europeans and their descendants, in laying aside the atrocious and exterminating policy which they have too long adopted towards those who have preceded them in the occupation of America, will yet be able to redeem their character by successfully pursuing a more liberal and humane system, which it is truly gratifying to observe they are at length, at least in some instances, disposed to substitute for it.

[To be continued.]

V. *A Catalogue of Comets. By* T. J. Hussey, *D.D., Rector of Hayes, Kent.*

[Continued from vol. iv. p. 352.]

[The Chronology employed is that of Petau or Petavius.]

A, the comet of 1680. B, that of 1652. C (Halley's), that of 1682. D, that of 1759. E, that of 1661. F, that of 1677. G, that of 1556. H, that of 1665. I, that of 1585. K, that of 1744. L, that of 1737.

Number.	Same as that of	Year of Appearance A. C.	Passage of the Perihelium in Mean Time at Greenwich.	Longitude of the Perihelium on the Orbit of the Comet.	Longitude of the Ascending Node.	Angle between the Perihelium and the Node.	Inclination.	Distance in the Perihelium, that of the Earth being = 1.	Log. Distance in the Perihelium.	Logarithm of the Mean Motion.	Eccentricity.	Motion.	Name of the Computer.
			d h m s	s ° ′ ″	s ° ′ ″	s ° ′ ″	° ′ ″						
440	C	1607	Oct. 26 3 51	10 2 16 0	1 20 21 0	3 18 5 0	17 2 0	0·586800	9·768490	0·30/393	...	R.	Halley.
			26 17 10 58	10 1 38 10	1 18 40 28	3 17 2 18	17 12 17	0·587974	9·769358	0·306091	0·9670888	R.	Bessel.
			26 21 44 39	10 1 3 40	1 17 48 40	3 16 45 0	17 20 0	0·58507	9·767208	0·309316	0·967391	R.	Halley.
441	...	1618	Aug. 17 3 3 0	10 18 20 0	9 23 25 0	0 24 55 0	21 28 0	0·512980	9·710100	0·394978	...	D.	Pingré.
442	...	——	Seen in November in the Constellation Centaurus. The observations are recorded by Kepler.										
443	...	——	Nov. 8 12 24	0 2 14 0	2 16 1 0	9 16 13 0	37 34 0	0·379750	9·579498	0·590881	...	D.	Halley.
		——	Nov. 8 24 54	0 3 5 21	2 15 44 10	9 17 21 11	37 11 31	0·389544	9·590556	0·574294	...	D.	Bessel.
444	...	1647	Seen in September and October in Coma Berenices, Bootes and Corona. The observations are recorded by Hevelius.										
445	B	1652	Nov. 12 15 41	0 28 18 40	2 28 10 0	10 0 8 40	79 28 0	0·847500	9·928140	0·067918	...	D.	Halley.
446	E	1661	Jan. 26 23 40 39	3 25 58 40	2 22 30 30	1 3 28 10	32 35 50	0.4485100	9·651772	0·482470	...	D·	Halley.
			Jan. 28 21 8 39	3 25 16 8	2 21 54 0	1 3 22 8	33 0 55	0·4427220	9·646131	0·490932	...	D.	Méchain.
447	...	1664	Dec. 4 11 52 39	4 10 41 25	2 21 13 55	10 10 32 30	21 18 40	1·0257550	0·011044	9·943562	...	R.	Halley.
448	H	1665	Apr. 24 5 15 19	2 11 54 30	7 18 2 0	5 6 7 30	76 5 0	0·1064900	9·027309	1·419164	...	R.	Halley.
449	...	1668	Seen in March in the Constellation Cetus. The observations are recorded in the Phil. Trans., *Mém. de l' Acad.*, &c.										
450	...	1672	Mar. 1 8 37 39	1 16 59 30	9 27 30 30	3 19 29 0	83 22 10	0·6973900	9·843476	0·194914	...	D.	Halley.
451	F	1677	May 6 0 37 49	4 17 37 5	7 26 49 10	3 9 12 5	79 3 15	0·2805900	9·448072	0·788020	...	R.	Halley.
452	...	1678	Aug. 26 14 3 39	10 27 46 0	5 11 40 0	5 16 6 0	3 4 20	1·2380200	0·092727	9·821037	...	D.	Douwes.
453	A	1680	Dec. 18 0 5 39	8 22 39 30	9 2 2 0	11 20 37 30	60 56 0	0·0061250	7·787106	3·279469	...	D.	Halley.
			17 23 9 39	8 22 44 25	9 2 2 0	11 20 42 25	61 6 48	0·0061750	7·790637	3·274701	0·9999107	D·	Halley.
			17 20 38 39	8 23 26 48	9 2 59 9	11 20 27 39	58 39 50	0·0065645	7·817202	3·234325	0·9997866	D.	Euler.
			17 23 54 39	8 23 43 0	9 1 53 0	11 21 50 0	61 20 20	0·0059200	7·772300	3·301678	...	D.	Newton.
			18 0 1 1	8 22 40 10	9 1 57 13	11 20 42 57	61 22 55	0·0060297	7·780295	3·289686	0·9999898	D.	Pingré.
			17 23 50 37	8 22 49 19	9 2 9 33	11 20 39 46	60 38 37	0·00623391	7·7947604	3·2679877	...	D.	Euler.
			17 23 46 9	8 32 49 5	9 2 9 29	11 20 39 36	60 40 16	0·00622236	7·7939551	3·2692957	0·999985417	D.	Euler.
454	C	1682	Sept. 14 7 39 39	10 2 52 15	1 21 16 30	3 18 23 45	16 56 0	0.583280	9·765877	0·311312	...	R.	Halley.
			14 21 21 39	10 1 36 0	1 20 48 0	3 19 12 0	17 42 0	0·582500	9·765296	0·312184	0·967392	R.	Halley.
			14 17 36 15	10 2 3 45	1 21 7 10	3 19 3 25	17 48 0	0·582428	9·7650424	0·3125647	0·9676763	R.	Burckhardt.
455	...	1683	July 13 2 49 39	2 25 29 30	5 23 23 0	2 27 53 30	83 11 0	0·560200	9·748343	0·337614	..	R.	Halley.

Number.	Same as that of	Year of Appearance A. C.	Passage of the Perihelium in Mean Time at Greenwich.	Longitude of the Perihelium on the Orbit of the Comet.	Longitude of the Ascending Node.	Angle between the Perihelium and the Node.	Inclination	Distance of the Perihelium that of the Earth being = 1.	Log. Distance in the Perihelium	Logarithm of the Mean Motion.	Eccentricity.	Motion.	Name of the Computer.
			d h m s	s ° ′ ″	s ° ′ ″	s ° ′ ″	° ′ ″						
456	...	1684	June 8 10 16 39	7 28 52 0	8 28 15 0	11 0 37 0	65 48 40	0·9601500	9·982339	9·986620	...	D.	Halley.
457	...	1686	Sept. 16 14 33 39	2 17 0 30	11 20 34 40	2 26 25 50	31 21 40	0·3250000	9·511883	0·692304	...	D.	Halley.
458	...	1689	Dec. 1 14 55 39	8 23 44 45	10 23 45 20	2 0 0 35	69 17 0	0·0168890	8·227604	2·618722	...	R.	Pingré.
459	...	1695	Nov. 17 ...	2 6 0 0	7 6 0 0	7 0 0 0	22 0 0	0·843480	9·925100	0·07773	...	D.	Burckhardt.
460	...	1698	Oct. 18 16 57 39	9 0 51 15	8 27 44 15	11 26 53 0	11 46 0	0·691290	9·839660	0·200638	...	R.	Halley.
461	...	1699	Jan. 13 8 22 39	7 2 31 6	10 21 45 35	3 19 14 29	69 20 0	0·744000	9·871570	0·152773	...	R.	La Caille.
462	...	—	Seen on October 25 in the Poop of Argo by Godfrey Kirch, at Guben in Lusatia.										
463	...	1701	Oct. 17 21 50 39	4 13 41 0	9 28 41 0	1 15 0 0	41 39 0	0·592630	9·772784	0·300953	...	R.	Burckhardt.
464	...	1702	Seen from February 22 until March 1, in all places having southern latitudes.										
465	...	—	Mar. 13 14 12 39	4 18 41 3	6 9 25 15	10 9 15 48	4 30 0	0·645900	9·810165	0·244881	...	D.	La Caille
			—— 13 14 33 22	4 18 46 34	6 8 59 10	10 9 47 24	4 24 44	0·646330	9·81079	0·243943	...	D.	Burckhardt.
466	...	1706	Jan. 30 4 22 39	2 12 29 10	0 13 11 40	1 29 17 30	55 14 10	0·425810	9·629218	0·516301	...	D.	La Caille.
			—— 30 4 56 39	2 12 36 25	0 13 11 23	1 29 25 2	55 14 5	0·426865	9·630291	0·514692	...	D.	Struyck.
467	...	1707	Dec. 11 23 30 0	2 19 54 56	1 22 46 35	0 27 8 21	88 36 0	0·85974	9·934368	0·058576	...	D.	La Caille.
			Dec. 11 23 43 26	2 19 58 9	1 22 50 29	0 27 7 40	88 37 40	0·85904	9·934013	0·059109	...	D.	Struyck.
			—— —— ——	2 17 4 0	1 22 8 0	0 0 4 56 0	88 50 0	0·86350	9·936262	0·055735	...	D.	Houttuyn.
468	...	1718	Jan. 14 23 39 0	4 1 30 0	4 8 43 0	0 7 13 0	30 20 0	1·02655	0·011380	9·943058	..	R.	La Caille.
			15 1 15 15	4 1 26 36	4 7 55 30	0 6 28 54	31 12 53	1·02565	0·010999	9·943629	...	R.	Douwes.
			15 7 38 0	4 1 3 40	4 8 21 0	0 7 17 20	30 48 30	1·02743	0·011753	9.942499	...	R.	Whiston.
469	...	1723	Sept. 27 16 10 39	1 12 52 20	0 14 16 0	11 1 23 40	49 59 0	0·99865	9·999414	9·961007	...	R.	Bradley.
			27 16 0 39	1 12 15 20	0 14 14 6	11 1 58 46	49 59 0	0·96980	9·986682	9·980105	...	R.	Struyck.
			23 20 29 3	1 12 35 12	0 14 10 2	11 1 34 50	49 55 25	0·999707	9·999870	9·9603233	1·019956	R.	Burckhardt.
470	...	1729	June 23 6 36 1	10 22 16 54	10 10 35 15	0 11 41 39	77 1 58	4·06980	0·609573	9·045769	...	D.	Douwes.
			25 11 6 39	10 22 40 0	10 10 32 37	0 12 7 23	76 58 4	4·26140	0·629552	9·015800	...	D.	La Caille.
			22 23 45 8	10 27 21 38	10 10 16 46	0 17 4 52	76 42 45	4·16927	0·620060	9·030038	...	D.	Maraldi.
			May 22 10 42 53	10 16 26 48	10 10 51 43	0 5 35 5	77 18 54	3·94927	0·596517	9·065353	...	D.	Kies.
			June 25 9 11 39	10 22 37 3	10 10 32 55	0 12 4 8	77 1 0	4·08165	0·610835	9·043876	...	D.	De Lisle.
			13 6 19 39	10 20 31 22	10 10 38 0	0 9 53 22	77 5 18	4·543496	0·6067570	9·0501878	1·0050334	D.	Burckhardt.

				12	4	10	0	10	20	27	36	10	10	38	0	0	9	49	36	77	5	18	4·0431	0·6067145	9·0502515	...	D.	Burckhardt.
471	...	1733	Seen from the Cape of Good of Hope and its neighbourhood during the month of May toward the North-west.																									
472	...	1737	Jan.	30	8	21	0	10	25	55	0	7	16	22	0	3	9	33	0	18	20	45	0·22282	9·347960	9·938188	..	D.	Bradley.
			June	8	7	38	0	8	22	36	39	4	3	53	43	4	18	42	56	39	14	5	0·86700	9·93802	0·05313	...	D.	Daussy.
473	...	1739	June	17	10	58	0	3	12	34	0	6	27	18	0	3	14	44	0	55	53	0	0·67160	9·827111	0·219462	...	R.	Zanotti.
				20	9	14	0	3	5	11	0	6	25	18	0	3	20	7	0	53	25	0	0·69614	9·842697	0·196083	...	R.	Zanotti.
				17	10	0	0	3	12	38	40	6	27	25	14	3	14	46	34	55	42	44	0·67358	9·828389	0·217546	...	R.	La Caille.
474	...	1742	Feb.	8	4	8	0	7	7	32	7	6	5	32	57	10	28	0	50	——			0·76550	9·883945	0·134211	...	R.	Le Monnier.
				8	4	21	9	7	7	33	44	6	5	34	45	10	28	1	1	67	4	11	0·765555	9·883976	0·134164	...	R.	Struyck.
				8	4	38	0	7	7	35	13	6	5	38	29	10	28	3	16	66	59	14	0·765680	9·884048	0·134058	...	R.	La Caille.
				8	7	30	0	7	7	39	10	6	5	42	41	10	28	3	31	66	52	4	0·765300	9·883832	0·134380	...	R.	Zanotti.
				7	10	39	0	7	10	49	23	6	9	32	7	10	28	42	44	61	13	44	0·752100	9·876224	0·145792	...	R.	Euler.
				7	21	51	0	7	7	33	28	6	5	47	22	10	28	13	54	68	14	0	0·768900	9·885870	0·131323	...	R.	Wright.
				8	5	18	0	7	7	26	23	6	5	29	28	10	28	3	5	67	11	9	0·766200	9·884342	0·133615	...	R.	Klinkenberg.
				8	7	12	0	7	7	37	50	6	5	41	32	10	28	3	42	66	51	0	0·765450	9·883917	0·134253	...	R.	Houttuyn.
				8	14	52	0	7	6	39	20	6	5	9	30	10	28	30	10	67	31	40	0·770055	9·886523	0·130344	...	R.	Barker.
475	...	——	Seen toward the South-east in the month of April, from the Indian Ocean.																									
476	...	1743	Jan.	10	21	15	36	3	2	58	4	2	8	10	48	0	24	47	16	2	15	50	0·838115	9·923303	0·075172	...	D.	Struyck.
				10	20	25	0	3	2	41	45	2	8	21	15	0	24	20	30	2	19	33	0·835010	9·921691	0·077593	...	D.	La Caille.
477	...	——	Sept.	20	21	16	0	8	6	33	52	0	5	16	25	8	1	17	27	45	48	21	0·521570	9·717310	0·384159	...	D.	Klinkenberg.
478	...	1744	Mar.	1	8	16	59	6	17	12	55	1	15	45	20	5	1	27	35	47	8	36	0·222060	9·346472	0·940420	...	D.	Betts.
				1	8	14	0	6	17	5	49	1	15	46	52	5	1	18	57	47	3	35	0·223220	9·348733	0·937029	...	D.	Maraldi.
				1	8	3	0	6	17	10	0	1	15	46	11	5	1	23	49	47	5	18	0·222500	9·347325	0·939141	...	D.	La Caille.
				1	7	59	0	6	17	17	30	1	15	51	0	5	1	26	30	47	18	0	0·221560	9·345491	0·941892	...	D.	Zanotti.
				1	8	59	0	6	17	19	26	1	16	5	24	5	1	14	2	47	49	53	0·221920	9·346196	0·940834	...	D.	Chéseaux.
				1	7	53	0	6	17	11	58	1	15	46	6	5	1	25	52	47	10	53	0·222220	9·346783	0·939954	...	D.	Euler.
				1	7	53	42	6	17	13	4	1	15	47	53	5	1	25	11	47	8	29	0·222229	9·346801	0·939927	...	D.	Pingré.
				1	7	42	9	6	17	14	36	1	15	49	27	5	1	25	9	47	17	38	0·222000	9·346353	0·940599	...	D.	Klinkenberg.
				1	8	57	19	6	17	16	16	1	15	49	30	5	1	26	46	47	14	10	0·221756	9·345875	0·941316	...	D.	Hiorter.
				1	7	50	39	6	17	29	0	1	16	3	0	5	1	16	0	47	50	0	0·220400	9·343212	0·945310	...	D.	Cassini.

VI. *On the Geological Evidence of the Advance of the Land at the Head of the Persian Gulf.* *By* Charles T. Beke, *Esq., F.S.A.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I HAVE now to trouble you with the following observations on the *Geological* evidence of the advance of the land at the head of the Persian Gulf, which you will oblige me by inserting in the next Number of the Philosophical Magazine, as a continuation of my paper on the *Historical* evidence on the same subject contained in the number of that Magazine this day published*.

Almost eighteen centuries have now elapsed since it was remarked by Pliny, in the passage which I have already cited†, that "in no part of the world did the land advance so greatly or so rapidly upon the sea" as at the head of the Persian Gulf. In making this observation that ardent and careful investigator of the phænomena of nature at the same time expressed his wonder (to use Mr. Lyell's paraphrastic translation‡,) "that the fluviatile matter was not swept away by the tide which penetrated far above the tracts where great accessions were made. This remark" (Mr. Lyell proceeds to say,) "proves that he had considered the different condition of rivers in inland seas, and those discharging their waters into the ocean." I may add that it proves even more evidently that the subject of the growth of land at the mouths of rivers was entirely familar to the natural historian, who (it is to be remembered,) was a native of Verona, a city at a short distance only from the shores of the Adriatic, and who, in the course of his active life, visited various countries bordering upon the Mediterranean—than which "no other inland sea affords so many examples of accessions of new lands at the mouths of rivers within the records of authentic history §"—

* Lond. and Edinb. Phil. Mag., vol. vi. p. 401.

† Lond. and Edinb. Phil. Mag. for February 1834, vol. iv. p. 109; and *Origines Biblicæ*, p. 21. "Nec ulla in parte plus aut celerius profecere terræ fluminibus invectæ. Magis id mirum est, æstu longe ultra id accedente, non repercussas." *Hist. Nat.* lib. vi. cap. xxvii.

‡ Principles of Geology, first edition, vol. i. p. 291. The passage here cited is repeated by the author in the second edition of his work, vol. i. p. 332; but in the third edition, published in August 1834, the paragraph in which it is contained, together with the above quotation from Pliny, which is also given in the former editions, are omitted, without any cause being assigned by the author for their suppression.

§ Principles of Geology, vol. i. p. 231; first edition.

and it further demonstrates that special attention had been devoted by him to the particular subject of the changes at the head of the Persian Gulf, since he pointedly contrasts what he conceives ought to have been the effect of the tide there, with the consequences of the absence of tides within the Mediterranean.

From the examination, therefore, of what is ascertained to have occurred at the mouths of the principal rivers flowing into this latter sea, resulting from the continuation of the process which was going on in Pliny's time and to his knowledge, we shall obtain the best means of forming an approximate idea of what also have been the actual changes at the mouths of those rivers which discharge themselves into the Persian Gulf. For the present means of this examination we are greatly indebted to the talents and research of the accomplished President of the Geological Society. It is, in fact, substantially effected in his able work to which I have so frequently referred*; and in citing that work once more as my authority, I shall content myself with adducing the single instance of the rapid formation of new lands at the mouths of the Po and Adige, as being that with which Pliny himself may, from the place of his birth, be presumed to have been better acquainted than with any other. We learn, then, that "from the northernmost point of the Gulf of Trieste, where the Isonzo enters, down to the south of Ravenna, there is an uninterrupted series of accessions of land, more than one hundred miles in length, which, within the last two thousand years, have increased from *two to twenty miles in breadth*†; that is to say, a formation of new land has within that period taken place, extending over an area of not less than *one thousand square miles.* Hence, even if we attach no importance whatever to Pliny's express reference to the *extraordinarily* rapid growth of the land in the Persian Gulf, we may, at all events, be permitted to calculate that during the same period the accession of new lands in that locality has at least been of equal magnitude with that which has taken place within the Adriatic; and if, further, we reckon that during the previous ages the rate of increase was no greater than during the last two thousand years, we shall have about 2300 square miles as the amount of the new lands which have been formed by the alluvial deposits of the Tigris and Euphrates since the date of the erection of the tower of Babel‡.

* Principles of Geology, vol. i. pp. 232—239; first edition.

† *Ibid.*, p. 236; first edition.

‡ According to Dr. Hales, the birth of Peleg, to which epoch I refer the dispersion of mankind (see *Orig. Bibl.*, p. 68), took place in the year 2745

It would, however, be most unreasonable to imagine that the quantity of detritus brought down by the rivers which flow into the Persian Gulf was not of far greater amount than that of the rivers of the Adriatic, when we consider that the Po itself is surpassed in size by many of the tributaries of the Tigris, whilst "the great river, the river Euphrates*," exceeds it at least five times in length.

It may be impossible to determine the precise proportion between the quantities of detritus transported by the two classes of rivers respectively; but it will not be unreasonable to assume (as a first approximation only) that equal quantities of detritus have been produced from equal spaces of the countries drained. Looking, then, to the basin of the Po and its tributaries, which extends southward to the confines of Tuscany, from which country it is parted by the Apennines; westward to the extremity of Piedmont, where the chain of the Alps surrounds it; and northward to where the Ticino, the Adda, and the Adige bring down the waters from a portion even of Switzerland and the Tyrol, we cannot compute its area at more than 45,000 square miles. On the other hand, if we regard the vast countries watered by the numerous and widely spreading affluents of the Euphrates and Tigris and the other rivers which discharge themselves into the Persian Gulf,—from the Arabian and Syrian deserts to the westward, as far as Asia Minor and Armenia towards the north, and thence to Koordistan and Khusistan towards the east; and even yet further in that direction beyond the Baktiari range of mountains;—it will be an error in defect rather than in excess to reckon their area at 270,000 square miles; that is to

B.C., or 4589 years since. If we take the date of the erection of Babel as usually computed, it will differ about 500 years from this, making the quantity of new land about 250 square miles less than as stated above.

This result is, *of itself*, sufficient to occasion very strong doubts as to the identity of the site of the Babel of Nimrod with that of the ruins in the neighbourhood of Hillah, and to induce us to look for it further to the northward. Its real position was most probably in the north-western portion of the land of Shinar, or Mesopotamia: see *Orig. Bibl.*, pp. 25 and 66.

* Does not the fact that in the times of Abraham, of Moses, and of Joshua, the Euphrates was styled "the *great* river," κατ' ἐξοχὴν, (see Gen. xv. 18; Deut. i. 7, xi. 24; Josh. i. 4.,) lead to the presumption that the Nile, a river far greater and much more remarkable than the Euphrates, could not have been known to the inspired writers at those early periods; thus further confirming the opinion advanced in *Origines Biblicæ* as to the original distinction between the *Mitzraim* of Scripture and the *Egypt* of Profane History? The former country, I will simply mention in this place, I conceive to have been altogether to the *eastward* of the Isthmus of Suez.

say, at six times more than the area of the countries drained by the rivers of the Adriatic.

If, then, in two thousand years a tract of 1000 square miles has been formed in the Gulf of Adria from the alluvium brought down by the rivers from 45,000 square miles of territory, it will (*cæteris paribus*) result, that at the head of the Persian Gulf the accession from 270,000 square miles of country will have been six times greater in amount; that is to say, a formation of not less than 6000 square miles of alluvial soil will have taken place within the last two thousand years: and if we compute the same rate of increase to have existed back from the building of Babel, this formation will have extended to 13,800 square miles of land.

In making the foregoing calculation it has been assumed that the circumstances are similar in both cases: this, however, is not precisely the case. The Adriatic is a gulf in a tideless (or almost tideless) sea: the rise and fall of the tide at the head of the Persian Gulf is (I believe,) as much as 8 or 9 feet at spring tides. From the effect, therefore, of the tide, and also from that of a current which sets across the head of the Persian Gulf from east to west, the accumulation at the mouths of the rivers would doubtless be checked, and a portion of the alluvium would be carried eastward and southward, and be dispersed in those directions over the bottom of the gulf. That such is actually the case is shown by the chart of this gulf lately constructed by the officers employed in its survey by the East India Company; from which it appears, that whilst along the north-eastern or Persian side of the gulf the depth, in great part, exceeds forty fathoms, along the whole of the Arabian or western and southern side it varies from twenty fathoms to shallows which are unnavigable, and which, to all appearance, will soon rise altogether above the level of the sea.

But, on the other hand, the much greater depth of the Adriatic, and of the Mediterranean generally, (which in many parts close to the shore is 2000 feet, and near Gibraltar even as much as 6000 feet deep*,) would tend very materially to reduce the rate of the *superficial* growth of the alluvial soil in that sea; and would thus—to a considerable extent, at least,—counterbalance the effects of the tide and current in the Persian Gulf.

On the whole, therefore, when we take into consideration the immense quantity of alluvial matter which is carried down into the Persian Gulf, we cannot hesitate to admit that Pliny

* Principles of Geology, vol. i. p. 237; first edition.

was not merely sincere in his belief, but also perfectly correct in his assertion that "in no part of the world* did the land gain so rapidly upon the sea;" and consequently we cannot, under any circumstances, be wrong in concluding that an accession of land very considerably greater than one thousand square miles has actually taken place at the head of that gulf within the last 2000 years. Hence it follows—and this is the point for which I need now principally to contend,—that the identifications of the various places mentioned by the ancients (and in particular by Nearchus) at the mouths of the rivers and along the coast at the head of the Persian Gulf, which have been made by geographers of modern times, and especially by the late Dr. Vincent and Major Rennell, must necessarily be erroneous †.

It is beyond the scope of the present paper to attempt to determine the correct positions of any of those places, or even to institute an inquiry into what may have been the direction of the coast line at the time when the voyage of Nearchus was undertaken, or at any other period of past history. I will, however, venture to offer the following suggestions, which may not be unworthy of the consideration of those who at any future period may feel inclined to devote their attention to the subject.

In the first place, then, it will be necessary to revert back to the time—whenever that time may have been,—when the Euphrates and Tigris discharged their waters into the Persian Gulf by entirely distinct and separate channels, each possessing its independent delta, as was formerly the case with the Po and Adige, the Ganges and Brahmapootra, and the Red River and Mississippi ‡.

* This assertion is of course to be understood with reference to those portions only of the globe which were actually known in those times; for the growth of the land in the Persian Gulf will not bear comparison with that which takes place at the mouths of the great rivers of the New World, or even of Eastern Asia. It would be an interesting subject of research to investigate the changes which have taken place at the mouths of the Indus since that river was visited by Nearchus.

† Mr. Carter, in his paper in the Philosophical Magazine (present series, vol. v. p. 248.) before alluded to, relies upon the extraordinary and (as it would appear) unfounded opinion asserted by Dean Vincent that Captain Howe's chart "explains the journal of Nearchus as perfectly as if it had been composed by a person on board of his fleet;" (Commerce and Navigation of the Ancients, vol. i. p. 423;) and that "the pilot on board Nearchus's ship steered exactly the same course" (*along the coast of the Delta*) "as MacCluer's Karack pilot, 2000 years afterwards" (*ibid.* p. 466).

‡ See Principles of Geology, vol. i. 252, first edition. As I have mentioned in *Orig. Bibl.*, p. 19, Mr. Lyell observes that "the union of the Tigris and Euphrates must undoubtedly have been one of the modern geographical changes on our earth."

Next will have to be investigated the particular operation of the causes which have produced the changes in the coast line; the principal of which causes are the transporting action of the rivers on the one hand, and the counteracting influence of the tides and current on the other. By the simple operation of the former of those causes, a progressive deposition of alluvial matter would have taken place, which would have been even and regular throughout: by the operation of the latter cause, however, that deposition would not merely have been obstructed, but its regularity and equal character materially affected; and in the same way as it has been shown * that the current from the westward in the Mediterranean has occasioned a general declination of the waters of the Nile from east to west, and has obstructed, and indeed in some cases closed up, the more eastern branches of that river, so by the operation of the current at the head of the Persian Gulf, would the western branches of the Euphrates gradually have become impeded, and finally closed: and further, as in the progress of time the deltas of the Euphrates and Tigris became continuous, the current of the former river would gradually have become so obstructed, that, in the end, it would (like the Rhine†) have lost its separate course to the sea, and would be destined to form a tributary to its more favourably situated rival.

It will likewise be necessary to ascertain what islands originally existed in the Persian Gulf below the mouths of the rivers; as their presence will very materially have influenced the direction and the rate of the formation of new lands in their vicinity‡.

In considering the progress of the changes in the configuration of the new lands which will thus have been formed, but which, in many cases, will also have been subjected to destruction from the effect of the currents of the rivers and the shifting of their channels, as well as from the effect of the tides and currents of the sea, it may perhaps, under any circumstances, be impossible to determine anything decisive as to their geographical outline and character at any former determinate period; but we may yet be enabled to obtain the idea (however faint) of the mouths of the Euphrates having first been impeded by shoals and sand-banks formed along the base of the separate delta of that river;—next, of the formation of one or more lakes through which the branches of

* *Orig. Bibl.*, pp. 172, 173.
† Principles of Geology, vol. i. p. 286.
‡ These islands will now, of course, form part of the main land.

that river would have had to force their way;—subsequently, of a portion of the Euphrates finding a partial course to the eastward, through the less obstructed channels of the Tigris, and of the consequently easier and more rapid victory of the sands over the sluggish, and at times almost stagnant waters of the former river;—then of the formation of marshes which would have been alternately flooded and left dry, as the waters of the Euphrates rose and fell;—till at length the union of the two rivers being perfected, by which completion of the process the united streams would roll together to the ocean, the separate course of the now tributary river, the Euphrates, would gradually become obliterated, and all traces of its existence at length be lost.

When the chorography of the countries in question shall have been investigated with reference to the change above alluded to, we shall be enabled to understand more fully and more satisfactorily the statements respecting them of the geographers and historians of antiquity: under any other point of view it is an interminable and hopeless task to attempt to reconcile their conflicting, and in many cases apparently totally contradictory, assertions.

London, June 1, 1835. CHARLES T. BEKE.

VII. *On the Silurian System of Rocks.* *By* RODERICK IMPEY MURCHISON, *F.R.S., Vice-President of the Geological and Royal Geographical Societies, &c. &c.**

GEOLOGISTS having long felt that the older sedimentary deposits required a systematic examination, I have devoted the last five years to the study of this class of rocks, hoping thereby to fill up certain pages which were wanting in the chronology of the science†. A table published last year was the first attempt to convey to the geological student a correct view of the thickness, variety of strata, and fossil organic contents of a vast system, which, though arranged by nature in the most lucid order of succession, had not previously been pointed out. These rocks, rising from beneath the old red sandstone in Herefordshire, Shropshire, Radnorshire, Brecknockshire, Monmouthshire, and Caermarthenshire, and each distinguished by separate and *peculiar organic remains,* were respectively named after those localities where each of them could be best studied, and their places in the series most clearly established. I have no change to an-

* Communicated by the Author.

† See Lond. and Edinb. Phil. Mag. 1832 to 1835, in the Proceedings of the Geological Society.

nounce in the order detailed in that table (see Lond. and Edinb. Phil. Mag., vol. iv. p. 370), but I wish to simplify it by the abandonment of double names, as applied to any one formation, and by the adoption of the names of those places only where the respective rocky masses lie in juxtaposition.

The names finally adopted, and which will be incorporated in a work now in preparation on this subject, are,

1. *Ludlow rocks*, divided into upper and lower Ludlow rocks, with a central zone of limestone: in this formation no change of name is proposed.

2. *Wenlock limestone* and shale (*equivalent, Dudley*).

3. *Caradoc sandstones*. This name, supplying the place of the Horderley and May Hill rocks, has been derived from the striking and well-known ridge of Caer Caradoc, on the eastern flanks of which, and lying between it and the Wenlock Edge, are exhibited those peculiar strata which are the equivalents of the shelly sandstones of Tortworth.

4. *Llandeilo flags* (preferred to "Builth and Llandeilo"). When this table is reprinted, there will naturally be found many additions to the organic remains, some identifications of British with foreign species, and numerous corrections.

Notwithstanding the adoption of these names, there was still required a comprehensive term by which the whole group could be designated, and at once distinguished from the *old red sandstone* above, and the *slaty rocks* below. Without such a collective name for the group, I found it impracticable to proceed with the work which I had engaged to complete, it being essential to the clear exposition of the subject, no longer to speak of these deposits as "transition rocks" or "fossiliferous grauwacké." The term 'transition' might indeed, have been retained, if for no other reason than to impress upon foreign geologists, (the Germans particularly,) how vast a difference exists between the geological horizon of the mountain or *carboniferous* limestone and that of the limestones of Ludlow and Wenlock, which are not only separated by many thousand feet of strata from the limestone of the carboniferous system, but, further, contain an entirely distinct class of organic remains. It was, however, utterly hopeless to use the word 'transition' in any definite sense as applied to these lower deposits, seeing the extent to which it had been abused. By some it was confined to those older rocks in which the earliest traces of organic remains were supposed to be observed, whilst others had more recently so expanded the meaning as to comprehend in it the whole of the carboniferous series! Thus at a period when, from the rapid advances of the science, it had become indispen-

sable to define the boundaries of groups naturally distinct from each other, dissimilar things were still confounded under one common name! and hence every geologist with whom I am acquainted had been for some time agreed upon the expediency of obliterating the term. The name 'transition' is, in truth, not applicable to any one class of stratified deposits in preference to another. Thus, for example, within the area of a map now preparing for publication and embracing parts of ten counties only, I shall be able to show *transitions* into every formation, beginning with the inferior oolite and terminating in descending order with the Llandeilo flags, many thousand feet below the old red sandstone; whilst the latter overlie other fossiliferous masses, the relative ages of which yet remain to be worked out! In various memoirs read before the Geological Society I have described these rocks as "fossiliferous grauwacké," but this term is in reality a misnomer, as the group contains few if any strata of the true grauwacké of German mineralogists. But whilst this system contains no such beds, it is underlaid and sometimes in discordant stratification, by a vast series of slaty rocks, in which much genuine grauwacké *is* exhibited. It was therefore manifest that if used at all in geological nomenclature, the term 'grauwacké' must be rejected as inapplicable to the first great system below the old red sandstone, and restricted to rocks which were *now* proved to be of much higher antiquity. My friend Professor Sedgwick will doubtless soon dispel the obscurity which hangs over these grauwacké rocks, with which his labours in Wales and Cumberland have so well enabled him successfully to grapple.

To return, however, to the system under review, I was urged by leading geologists both at home and abroad to propound an entirely new name for it. In consonance, therefore, with those views which have rendered the names used by English geologists so current throughout the world, I venture to suggest, that as the great mass of rocks in question, trending from south-west to north-east, traverses the kingdom of our ancestors the Silures, the term "Silurian system" should be adopted as expressive of the deposits which lie between the old red sandstone and the slaty rocks of Wales, including, as above detailed, the Ludlow, Wenlock, Caradoc, and Llandeilo formations. One of the largest of these formations, to which, indeed, the Llandeilo flags are frequently subordinate, has been named after the bold and picturesque ridge of Caer *Caradoc* in Shropshire.

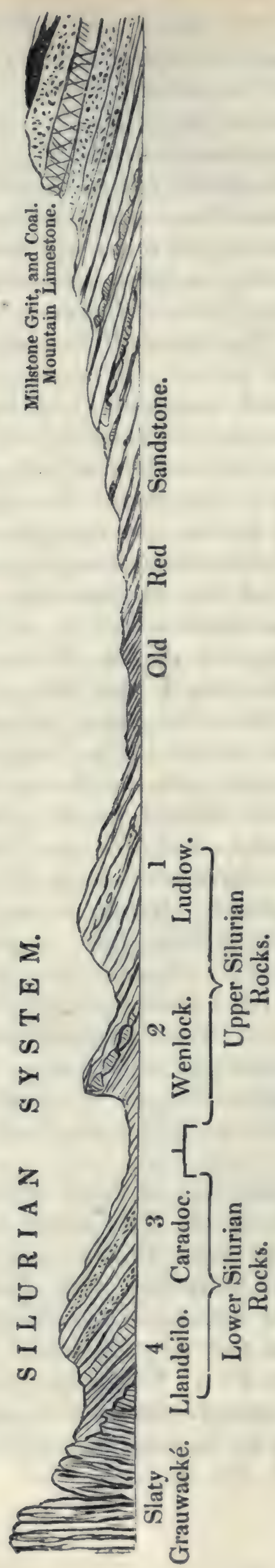
I further propose that the system be subdivided into "Upper" and "Lower Silurian rocks," the former em-

bracing the deposits of "Ludlow" and "Wenlock," the latter those of "Caradoc" and "Llandeilo." By this arrangement the observer will not be embarrassed when he finds that certain typical strata have disappeared. Thus, for instance, when the zones of limestone thin out, by which the formations of Ludlow and Wenlock are subdivided, it is no longer practicable to mark lines of separation between them. Under such circumstances the united mass will simply be described as "*Upper Silurian rocks*," whilst, wherever similar causes prevent the separation of the "Caradoc sandstones" from the "Llandeilo flags," the two will be termed "*Lower Silurian rocks*."

A wood-cut is annexed, explanatory of the manner in which these deposits are connected with the old red sandstone above them, with each other, and with the slaty grauwacké rocks beneath them. Although the lower Silurian rocks are frequently unconformable to the schistose grauwacké, as represented in the wood-cut, there are situations in which a passage from the one to the other may be detected.

Having alluded to a work which is in preparation upon the geology of the country of the Silures, and of several of the surrounding counties, I may take this opportunity of acquainting those friends who have fostered the undertaking, that the author is unceasingly occupied in promoting its completion. Geologists are not, I am sure, among those who feel surprise at the delay; for they well know that so large an area of country, and one so little previously examined, could not have been adequately described without several years of hard labour. In the mean time I may state that a map is now constructing, being a precise reduction of the Ordnance Maps, extending over the greater part of ten counties, all of which have been geologically coloured in the field; together with about twenty plates of engravings of fossils, many of which are of new species, and a vast number of coloured sections, vignettes, &c., all of which will, I trust, be completed so as to enable me to lay the work before the public in a few months.

P.S. Authorities differ in opinion concerning the exact geographical boundaries of the kingdom of the Silures. Cluverius and Camden state that it embraced the greater part of South Wales, including also Monmouthshire and Herefordshire. Hume speaks of the Silures as a nation inhabiting the banks of the *Severn*, whilst others think that their chief river was the *Wye*. The Roman historians afford no correct account of the geography of this region, but they assure us that the Silures were, of all the nations of South Britain,



SILURIAN SYSTEM.
Slaty Grauwacké.
4 Llandeilo.
3 Caradoc.
Lower Silurian Rocks.
2 Wenlock.
1 Ludlow.
Upper Silurian Rocks.
Old Red Sandstone.
Mountain Limestone.
Millstone Grit, and Coal.

the most powerful and warlike, impatient of slavery, and of great intrepidity. Such was their confidence in their gallant leader Caradoc (Caractacus), and so exasperated were they at the saying of the Emperor Claudius, "that the very name of Silures must be extirpated," that they carried on a stubborn war, not only under Caractacus, but long after his capture, defeating the legion under Manlius Valens, and wearying out the indefatigable Pro-Prætor Ostorius, who died when spent with the difficulties they opposed to him. Veranius, who commanded under Nero, attacked them in vain, and they were only finally subdued by Julius Frontinus in the time of Vespasian! British geologists, therefore, will not doubt that "Siluria" is a name entitled to be revived, when they are reminded that these struggles of their ancestors took place upon the very hills which it is proposed to illustrate under the term "Silurian system." Antiquaries are not agreed concerning the exact spot in which Caractacus made his last stand against the Romans. Camden has unquestionably erred in supposing it to have been on Caer Caradoc, an error into which he was doubtless led by the hill bearing the name of the great chief. The existence of a river at times deep and rapid is pointedly mentioned by Tacitus, and there is no such feature at the base of Caer Caradoc. The site of this battle is now most generally supposed to be Coxwall Knoll*, near Leintwardine, about 10 miles west of Ludlow, and on the left bank of the Teme, to which stream the hill opposes a precipitous face. Having examined this locality with the eye of a soldier, and with the words of Tacitus in my recollection, I am disposed to doubt the accuracy of the conjecture, and I may, on some other occasion, point out the reason for this dissent, and suggest the position which the wily Caractacus may have occupied. This, however, is of no interest to the geological question, for even if Coxwall Knoll be the spot, it is in the very heart of the "Silurian system" of rocks. Although a profound antiquary and one to whose erudition I ought perhaps instantly to defer, is of opinion that the country of the Silures never extended so far to the north as Caer Caradoc and Wenlock Edge, I am still (until decisive counterbalancing evidence be produced,) disposed to think that the territory over which this warlike race exercised an influence must have been prolonged to the gorge of the Severn. It appears highly improbable that the north-eastern portion of a hilly system, which running from south-west to north-east has its natural termination in the parallel ridges of Caer Caradoc and Wenlock Edge, should no thave been occupied or controlled

* See Ordnance Map.

by the same powerful people who possessed all the remainder of the chain, particularly as the Severn forms a well-defined natural boundary to the ridges in question. But after all, should antiquaries prevail in abstracting this hilly tract of South Salop in which the "Silurian system" of rocks is so well displayed, from the domains of old Caradoc, ample space is still left in Herefordshire, Radnorshire, Brecknockshire, and Monmouthshire, to sanction the use of the name proposed. In allusion to this term I have only further to add, that it is to be hoped that no naturalist will, from its sound, fall into the mistake of an early English writer who is ridiculed by Camden for having misapplied the line of Juvenal,

> "Magna qui voce solebat
> Vendere municipes fracta de merce Siluros,"

supposing that the British captives were exposed to sale at Rome, when the poet spoke of *fishes*, and not of men! My geological readers do not require to be told that there are no fossilized remains of the "*Silurus*," or bony Pike, in these deposits, since M. Agassiz will afford us very different names for the ichthyolites of the Ludlow rocks.

VIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

PROCEEDINGS at the Annual General Meeting, 20th February 1835.

The various official Reports having been received, the President delivered the following Address:

GENTLEMEN,

In the Report which has just been read to you, it is stated in what manner the Council have adjudicated the proceeds of the Wollaston Fund for the present year. To carry into effect that award is the pleasing duty which I now have to perform. It is to me as well as to Mr. Mantell a subject of deep regret that he cannot attend to receive the prize in person. I shall deliver it, with your permission, to Mr. Lyell, who will officiate as his representative on this occasion.

Mr. Lyell,—In the name of the Geological Society I beg to commit to your care the proceeds for the present year of a fund bequeathed to us by one of the most eminent philosophers to whom this country has given birth, and by him directed to be applied to the furtherance of geological science. The Council are of opinion that they cannot upon the present occasion more conscientiously discharge the duty imposed upon them, than by awarding this prize to Mr. Mantell. Zealously engaged as he is in the practice of an arduous profession, we, his fellow labourers in this Society, have witnessed with great satisfaction for a series of years his unceasing endeavours to unravel the geological phænomena of the interesting district around him. By long experience Mr. Mantell has acquired

so much skill in the dissection, if I may call it so, of fossils, from amidst the matrix that conceals them, that many of the finest specimens in that rich and beautiful collection which his liberality has opened to the public, may almost be considered as the works of his hand: his active researches in different branches of natural history, and more especially his investigations in comparative anatomy, pregnant with inferences and analogies illustrative of the early history of our planet, are convincing proofs of the energy and activity of his mind, of his determined love of knowledge, of his parsimony of time, of his unbounded prodigality of labour. His discovery of the Iguanodon in 1828, and his determination of the place which it occupied in the scale of animated beings, prove his sagacity and acquaintance at that early period with the principles of anatomical and zoological science. So strange to the eye of the naturalist were the first discovered remains of this gigantic animal, that Cuvier himself knew not to what genus recent or fossil they could with any propriety be assimilated. Mr. Mantell discerned its relation to the Iguana, and the fortunate and wholly unexpected disinterment which has lately taken place at Maidstone of a considerable assemblage of the fossil bones of this creature*, together with the impression of a tooth, confirm in many respects his early conjectures. The Hylæosaurus, also, another extinct genus, was first brought to light by Mr. Mantell's labours, and correctly illustrated by the application of his anatomical knowledge†. I will only add, in the way of congratulation, that his perseverance in these interesting researches is not remote from the habits, or at variance with the duties of his profession; for, as you are well aware, the examination of the human body is one department of that wide and beautiful field of comparative anatomy in which he finds amusement; and to become well acquainted with the œconomy and physiology of man, it is necessary to study the structure of other animals. We hope and trust, that Mr. Mantell may long possess health and strength to advance further and further still in his honourable career, and we request you, Sir, to convey to him our earnest wish that in every department of life he may meet with that success which his industry and talents so eminently deserve.

The President then presented, in the name of the Society, a purse of guineas to Mr. Lyell to be by him conveyed to Mr. Mantell, and informed him that the remaining portion of the proceeds of the fund had been laid out on a Medal, which would likewise be forwarded to him.

The Meeting then proceeded to ballot for the Officers and Council for the ensuing year; when the following gentlemen were declared duly elected:—

Officers.—*President*, Charles Lyell, jun. Esq. F.R.S. & L.S.: *Vice Presidents*, Sir Philip de Malpas Grey Egerton, Bart. M.P. F.R.S.; Roderick Impey Murchison, Esq. F.R.S. & L.S.; Edward Turner, M.D. F.R.S. L. & E. Professor of Chemistry in the Uni-

* [See Lond. and Edinb. Phil. Mag., vol. v. p. 77. Edit.]

† [See vol. ii. p. 150—151. Edit.]

versity of London; Henry Warburton, Esq. M.P. F.R.S. & L.S.: *Secretaries*, William John Hamilton, Esq.; Woodbine Parish, jun. Esq. F.R.S.: *Foreign Secretary*, Henry Thomas De la Beche, Esq. F.R.S. & L.S.: *Treasurer*, John Taylor, Esq. F.R.S.

Council.—George William Aylmer, Esq.; Francis Baily, Esq. F.R.S. & L.S.; Arthur K. Barclay, Esq.; William John Broderip, Esq. F.R.S. & L.S.; William Henry Fitton, M.D. F.R.S. & L.S.; George Bellas Greenough, Esq. F.R.S. & L.S.; Henry Hallam, Esq. F.R.S.; John Forbes Royle, Esq. F.L.S.; Rev. Adam Sedgwick, Woodwardian Professor in the University of Cambridge, F.R.S. & L.S.; Lieut.-Col. W. H. Sykes, F.R.S. & L.S.; John Henry Vivian, Esq. M.P. F.R.S.; Sir Richard Vyvyan, Bart. M.P. F.R.S.; Rev. James Yates, F.L.S.

The following Address was afterwards delivered by the President, G. B. Greenough, Esq. F.R.S., &c.

Gentlemen,

I rejoice to see you assemble in these long-desired apartments: the increased facilities now afforded to us of pursuing the objects of our institution demand on our part a corresponding increase of exertion. Let it not be imagined that in accepting a boon from the Government we have not incurred an obligation. Our claim rested entirely on the sense which the public entertain of our deserts. The full and accurate knowledge, which it has been our aim to acquire and publish, of the subterranean resources of all parts of the world, cannot fail to be useful: so long as the utility of our establishment is distinctly felt, independently of its bearings upon what to us appear the higher considerations of science, so long may we rely on the continuance of public support; but our claim to that support would vanish at once, if we should relax our exertions, and fail to realize those expectations which our hitherto well-sustained activity has kept alive in the breast of our benefactors.

Gentlemen, you are well aware that all the accommodation which you now enjoy has not been derived, however, solely from the beneficence of the Government, set in motion by the Royal Society. On our first taking possession of this house the repairs and alterations occasioned on our part an immediate outlay of 1500*l.*, and 1000*l.* more have now been expended in improving and furnishing the new apartments, and in availing ourselves of such other advantages as their acquisition has enabled us to command. The Council considering it imprudent that so large a sum should be deducted from the capital of the Society, 500*l.* of that amount have been provided by a voluntary subscription.

The alterations planned by Mr. Decimus Burton have been executed under his direction within the specified time, and in accordance with the estimates. With what skill they have been executed has not escaped the notice of the Building Committee; with what success is apparent to you all. You have been informed that Mr. Burton has declined to accept any remuneration for his professional services, but I cannot deny myself the pleasure of again recording this new instance of his public spirit and characteristic liberality.

The concluding part of the third volume of our Transactions is in the press, and the recent arrangements of the Council induce me to hope that not this part only, but also the first part of the fourth volume will be ready for delivery in the course of the present year.

The number of our Fellows has received an addition of forty-two and a diminution of ten; of those whom we have lost, three only are known to me as Contributors to the Transactions or as Geological Authors.

To the liberality of Mr. Matthew Culley, the chief of a family greatly celebrated for the practical improvements they have introduced into agriculture, our Museum is indebted for a large series of primitive rocks collected in the remoter parts of Sutherlandshire, as also for some fine specimens of the fossil fishes which occur at Banniskirk, situated eight miles south of Thurso in Caithness. The geological relations of these last have been since investigated by Mr. Murchison and Professor Sedgwick, and their zoological characters determined by M. Agassiz*. Mr. Culley also transmitted to us an account of the prodigious power occasionally exerted by rivulets when swoln by heavy rain.

Major James Franklin was a younger brother of Captain Sir John Franklin, R.N.; he commenced the survey of Bundelcund in India in 1813, and continued it during four years, after which he joined the army in the field, under the Marquis of Hastings. In December 1818, Captain Franklin returned to the duties of his survey, and shortly afterwards was promoted to the rank of Assistant Quarter-Master General. In 1820, he commenced at Calcutta the construction of his maps. He afterwards made a careful survey of Sincapore and the adjacent strait. Having repaired to England in 1823 for the benefit of his health, he soon learned by communication with members of this Society, how much the value of his surveys would have been enhanced by geological descriptions. On his return to India in 1826, his first care was to supply this desideratum: he solicited, though unsuccessfully, the appointment of Geologist to the trigonometrical survey then carrying on in India, and strenuously recommended that all officers employed on that service should be qualified and encouraged to collect materials for the construction of a geological map of the entire Peninsula. Declining health brought him again to England in 1829, where he remained till his death, which took place in the summer of last year. He transmitted to us a paper on the geology of a portion of Bundelcund † and other districts in Central India; and to the Asiatic Society of Bengal a description of the Diamond Mines of Panna, published in the Transactions of the respective Societies. Among his MSS. have been found Observations on several Iron Mines, and on the mode in which the ore extracted from them is manufactured in Central India, together with an account of different beds of coal in that country.

* Proceedings of the Society, May 1829.

† Transactions of the Geological Society, 2nd Series, vol. iii. Part I.

Mr. James Hardie evinced in early life a taste for natural history. Educated at Edinburgh, he founded the Plinian Society in that city, and contributed largely to its Museum. In 1784 he embarked for India, and served in the Bheal campaign; he was afterwards appointed the Residency Surgeon at Odeypoor, and made a survey of the neighbourhood. In his visits to Calcutta he acquired the friendship of the most distinguished geologists of the East; he became a member of the Medical and Physical Society, and contributed many papers to their Transactions. A survey of his route on one occasion from Calcutta to Bombay, and thence to Odeypoor, will be found in the Transactions of the Asiatic Society, of which he was also a member. In 1830, he made a voyage round the Indian Archipelago, with a view to the recovery of his health. He passed six months at Java, and paid much attention to the geology of that island. On his return to Scotland he presented his collection to the Museum at Edinburgh. Professor Jameson recommended him to the East India Company as a fit successor to the appointment held by the late Dr. Turnbull Christie. Mr. Hardy repaired to Paris in 1833 for the purpose of prosecuting his geological studies, and died there in May following at the age of 31.

In reviewing the geological labours of the year I shall advert principally, but not exclusively, to those of our own members, and the order of precedence will be regulated by the nature of the respective papers, without any reference to date*.

Mr. Murchison, in prosecution of the work in which he has been so long and actively engaged, has communicated to us his observations on the detritus that covers the old red sandstone in Herefordshire and its vicinity. All the detritus, he says, seems to be derived from neighbouring rocks. Granite boulders are nowhere found within its area, but they occur of large dimensions and of various sorts upon its northern confines; he states generally that they appear to have come from the North. Many, if not all of them, may I believe be identified with the granitic rocks of Westmoreland and Cumberland. Several of those I have observed on the north of Shrewsbury have the character of the Irton rather than of the Shap granite. The detritus of the old red sandstone is ascribed to the operation of different causes, some of which may perhaps require further study.

From Mr. Strickland we have received three communications respecting certain bones of extinct quadrupeds associated at Cropthorne in Worcestershire, with existing species of shells. On a base of lias clay reposes a layer of fine sand containing twenty-three species of land and freshwater shells, together with rolled and broken bones of the Ox, Deer, Dog, Bear, and Hippopotamus. Upwards this sand passes into gravel undistinguishable from the so-called diluvium. These shells are found at five or six different localities within the Vale of Evesham. Two of the species are thought to be

* [Abstracts of the papers read before the Geological Society during the past year, and referred to in Mr. Greenough's Address, have been given in Lond. and Edinb. Phil. Mag., in the last two and present volumes,—iv. v. vi. Edit.]

extinct. The inference drawn from all the phænomena is that this deposit occupies the site of a former river-bed or lake; that since its formation mammiferous animals have migrated more than molluscous, and that the climate has remained nearly stationary.

Mr. Edward Charlesworth has placed in our Museum some valuable specimens collected at Sutton in Suffolk, valuable because they establish the existence of similar phænomena there also. The specimens consist of bones, (one of which appears to belong to the elephant,) and of freshwater shells, including an extinct species of Cyclas. Five or six feet below the surface a layer of calcareous nodules may be traced, he says, for about half a mile along the banks of the Stour; the nodules are very numerous and found only with the shells. Let us hope that the interest excited by these notices may lead some competent naturalist to undertake a detailed examination of the "Crag," our imperfect acquaintance with which, again and again pointed out from this Chair, continues to be what I may justly call one of the Opprobria of English Geology.*

The changes which have taken place in the boundaries of the land and sea on the north-eastern coast of Kent have been brought under our notice by Mr. Richardson.

Mr. Wetherell has transmitted to us a detailed account of a Well recently sunk on the south side of Hampstead; its depth is 330 feet; under 285 feet of clay was found a rock five feet thick, which in external character and fossil contents agrees with the Bognor, and rests on plastic clay. At the depth of 160 feet specimens were brought up of what would be called in Sheppy Island fossil fruits: the local distribution of these remarkable bodies is very limited. From the examination of Mr. König, it would appear that a few only are of vegetable origin. By far the greater part belong to a distinct class, which, in Mr. König's new division of the natural order of Polypi, are distinguished by the appropriate name of Carpomorphi.

Mr. Rofe has found in the neighbourhood of Reading the Bognor rock, occupying the same position which Mr. Wetherell assigns to it at Hampstead. It is strange that in a quarry of which so many sections have been given, this rock should have remained so long unnoticed. Mr. Rofe has also observed, that in the wells at Reading the level of the water depends not on the Kennet which is the nearest river and flows over tenacious clay, but on the more distant Thames which flows over gravel resting upon chalk.

M. Boué has recently pronounced the plastic clay formation destitute of fossils, but it certainly does contain them in England.

Mr. Woodbine Parish has detected chalk in a part of the Sussex coast, where its existence had not been before observed. He has traced it from Felpham to the distance of about a mile; it runs in the direction of Middleton, where chalk marl has been obtained at low water.

Dr. Mitchell has pointed out to us in what respect the chalk of

* [Our next (August) Number will contain a paper on the Crag, by Mr. Charlesworth, supplying some of the present desiderata regarding that formation.—Edit.]

the North of England differs from that of the South. The difference consists in its greater hardness, its occasional redness, its well-defined stratification, the absence of flint nodules in its upper portion, and the continuity of layers of flint in its lower; in many of these characters it resembles the chalk of Antrim.

The occurrence of Hippurites in the chalk of Sussex appears to require confirmation.

A well recently sunk at Diss in Norfolk, after penetrating two beds of clay and sand, the aggregate thickness of which amounted to 100 feet, penetrated through the great body of the chalk, which proved to be 500 feet in thickness; the tools passed afterwards through five feet of sand, when water flowed in, and rose to within 47 feet of the surface. Mr. Taylor, our Treasurer, to whom we are indebted for this information, states that the proprietor was encouraged to persevere in so expensive an undertaking by the account which he had seen of a similar well in Cambridgeshire, drawn up by Mr. Lunn and published in our Transactions. The confidence which he placed in that analogy (though the places are many miles distant from one another,) has been fully justified by the event, and the success of the experiment furnishes a striking example of the value of records of this nature.

Dr. Fitton's paper on the strata of the South-east of England, between the chalk and Oxford oolite, together with the memoirs of Dr. Buckland and Mr. De la Beche on the coast of Dorsetshire, are in great part printed.

The Wealden and Purbeck beds are generally supposed to have been deposited in an estuary, but what may have been the form, direction and extent of that estuary, it is not easy to conjecture. Similar beds occur at Lady Down in Wiltshire, and at Swindon; and the Vivipara, one of their characteristic fossils, appears towards the top of Shotover. The discovery of this formation in the neighbourhood of Beauvais excited much interest among the naturalists, assembled there about three years ago, and several species of the Wealden fossils seem to prove its existence at Loch Staffin in the Isle of Sky.

And here, while considering the district of the Weald, allow me to direct your attention for a moment to a subject temporarily, and let us hope, only accidentally connected with it. I refer to the repeated, though slight shocks of earthquakes, which have been experienced during the last eighteen months in the neighbourhood of Chichester. When we recollect the prodigious area over which the earthquake of Lisbon was felt, comprehending one fourth of the entire northern hemisphere, we can hardly suppose it possible that the cause of such phænomena can be seated so near the surface of the earth as to come at all within the range of our observation. It is, however, a singular coincidence, that in 1734, 1747, 1749, 1755, and the three following years, earthquakes were felt in this same part of England, and that on those occasions, as well as now, the direction of the shocks is supposed to have nearly coincided with the great line of fault.

On the oolite district we have received no additional intelligence. The boundaries of the several clay-beds in the midland and northern

counties is as yet ill defined, and the same remark will apply to many of the stone-beds in Lincolnshire, Leicestershire, and Rutlandshire. The anomalies of the Stonesfield slate are still unexplained, and it is very desirable that figures should be published of the fossil remains found in it, and more especially the coleopterous insects.

Mr. Murchison has discovered an outlier of lias at Longdon Hill, near Upton, and improved the boundary line of that formation in the neighbourhood of the Severn. The lias marl or lower lias rock, ill exhibited along the Yorkshire coast, and on the coast of Dorsetshire imperfectly developed, in Worcestershire and Gloucestershire swells into importance, and may be traced continuously for a distance of twenty or thirty miles, passing insensibly here, as in Germany, into the red marl.

A much more unexpected discovery has been made, by the same indefatigable observer, of the same beds in another situation, viz. between the Hawkstone Hills and the towns of Whitchurch and Market Drayton, seventy miles apart from the lias range in the midland counties. This outlier is known to extend about ten miles in length, and from four to six in breadth; its greatest extent is from north-east to south-west; its western boundary is obscure. The strata have a slight dip inwards towards a common centre. The visit of a geologist to this spot has had the effect of stopping an inconsiderate speculation; the bituminous character of the lower beds had been supposed to indicate the proximity of coal, and a shaft had been sunk to the depth of three hundred feet in search of that mineral, in a place which I need hardly say afforded not the slightest chance of success.

The escarpment here exposed presents to view the junction of the middle and lower part of the formation. With most of its fossils we are familiar, but six or seven of the species met with are new in England, though some of these occur in a corresponding position at Brora in Sutherlandshire, and others are figured in the valuable work which Mr. Zieten is now publishing on the Organic Remains of the Kingdom of Wurtemburg. A shaft sunk at Kentsborough has reached the brine springs and gypsum of the subjacent formation.

Mr. Williamson, jun., has given us a detailed account of the lias near Scarborough, to which I shall have occasion to advert again.

Mr. Murchison has described to us the new red sandstone on the confines of England and Wales; the formation here, as in the north-eastern counties, may be divided into

1. Red and green marl;
2. Sandstone and conglomerate;
3. Calcareous and dolomitic conglomerate;
4. Lower red sandstone:

corresponding to 1. the Keuper; 2. Bunt sandstein; 3. Zechstein; 4. Rothe todte liegende of the Germans.

Limestone is sparingly distributed in this district. No trace has been met with of the muschelkalk, and the magnesian limestone occurs only in the shape of a sandstone conglomerate.

1. The red and green marls afford brine-springs in Gloucestershire, Worcestershire, Shropshire, and Cheshire, but with a small proportion of gypsum.

2. The beds immediately beneath are largely developed in the north of Shrewsbury, in Staffordshire and Salop; the district which they occupy is wild and barren, owing to the prevalence of decomposed quartzose conglomerates. Where sand predominates it is more fertile, and the country assumes a character perfectly distinct from that which belongs to the upper and lower beds.

3. The fragments of the calcareous conglomerate are occasionally of oolitic limestone, sometimes of old red sandstone, or of some member of the coal series. This division is unimportant on the east of Coalbrook Dale, but in the western part of Salop it swells out and corresponds with the dolomitic conglomerate of Bristol. At Cardeston it is from eighty to one hundred feet in thickness; its escarpment, with partial interruptions, may be traced from Alberbury and the Brythins round the carboniferous promontory of Salop and the Clent Hills, and it forms a distinct ridge between Bridgenorth and Kidderminster.

4. Beneath this, in Salop and Worcestershire, is found a thick deposit of reddish sandstone (rothe todte liegende); it passes upwards into dolomitic conglomerate, and downwards into the coal-measures in conformable beds, so that there is great reason to suppose that unwrought coal lies beneath. Mr. Murchison has determined the extent of this rock, which occasionally towards the bottom contains trappean conglomerates like those of Heavitree in Devon, or feldspathic rocks like those of the Malvern and Abberley range.

The dolomitic conglomerate just mentioned has in another county engaged the attention of the Rev. David Williams: he has discovered for the first time Saurian reptiles in this deposit. Mr. Conybeare had before noticed the occurrence of a part of the skeleton of a supposed gavial near the bottom of the red sandstone in Worcestershire.

The obscurity which for so many years continued to involve the red sandstone deposits both in England and on the Continent was first cleared away by Professor Sedgwick. We now know that the proper position of the rothe todte liegende of the Germans is immediately beneath the magnesian limestone, and that it is the same rock which Mr. Smith described as the Pontefract sandstone. It is much to be regretted that Mr. Hoffmann in the beautiful map and sections which he has published of North-western Germany, has designated by one name, as if they were only parts of one and the same deposit, the red sandstones and conglomerate that lie under the carboniferous series, and those that lie above it. This grouping together of formations so widely separated in nature is very objectionable. With a view to distinctness it is essential that the rothe todte liegende shall not be classed as heretofore, sometimes with the old red sandstone, sometimes with the new, sometimes with both, sometimes with the coal-measures, but that it should hold the rank of a substantive and independent formation. The barbarous phrase,

which has just been employed to designate it, though tolerated in Germany, will never, it is hoped, be naturalized here. The name by which it is known at Tarnowitz is too local. Many other names have been proposed when one would have sufficed. By M. de Beaumont it is called Grès des Vosges; by Mr. Smith, the Pontefract rock; by Professor Sedgwick, the Plumpton; and by Dr. Hibbert, the Roslyn sandstone. Assuming that all these names are synonymous, the last appears for many and obvious reasons the most worthy of adoption.

This important rock, which, in richness of soil, in undulation of surface, and in the luxuriant growth of its timber, closely resembles in many places the old red sandstone of Herefordshire, lies in general, sometimes conformably, sometimes unconformably, upon coal-measures, and even contains occasionally beds of that substance. But in the neighbourhood of Shrewsbury, and also at Tasley and Coughley near Bridgnorth, Mr. Murchison has shown that this lower red sandstone overlies unconformably and passes down into a zone of coal-measures containing a peculiar fresh-water limestone. The great coal-beds of Broseley and Coalbrook Dale are wrought beneath it.

I had occasion myself to observe during the last summer, in the neighbourhood of Nuneaton in Warwickshire, a limestone similar in aspect, lying under a two-foot bed of sulphureous coal. The limestone is about fourteen inches in thickness, and exhibits veins of galena in calcareous spar. At the first pit I visited there was but one bed of limestone, but at another on the same estate is a second bed which also contains galena, and on its surface numerous impressions of plants. The interval between the two beds is occupied by a sandstone not unlike the Pennant rock in appearance, and what is here called a chance coal. Immediately beneath the lower one is another bed of coal four feet in thickness.

In three papers which Mr. Conybeare has lately published on the relations of our principal coal-fields*, he considers it probable that coal will be discovered hereafter in many districts as yet unexplored. He dwells upon our uncertainty as to the boundary of the carboniferous beds in the midland counties, and recommends that a survey should be undertaken expressly with a view to determine this problem: "It is little to the credit," he observes, "of a nation like ours, so peculiarly dependent on this branch of her mineral resources, that we continue to acquiesce in a state of ignorance so easily removed. We here see a strong instance of our want of a regular school of mining, such as is possessed by many countries."

Mr. Elias Hall has published a geological map of Lancashire, a county hitherto comparatively neglected, and, I am sorry to add, very indifferently represented in all the geological maps of England. Mr. Hall is entitled to great praise for his intrepidity and perseverance; had he not possessed these qualities in an eminent degree,

* Lond. and Edinb. Phil. Mag. and Journ. of Science, vol. iv. pp. 161 and 346, vol. v. p. 44.

he never would have entered, as it were alone and single-handed, on so irksome and laborious an investigation. That the work is in many respects imperfect must be admitted, but considering the apparent disproportion of his means to his end, it is surprising that the author should have achieved so much: what he has left incomplete or inaccurate will be readily supplied and corrected by the supplemental labours of more fortunate observers, when the physical features of this extensive tract shall have been accurately delineated by the Ordnance Department.

For a detailed account of the carboniferous tracts in Salop and the adjacent counties we are indebted to Mr. Murchison. The following are the conclusions which his paper tends to establish.

1. In the Shrewsbury coal-field the presence of a younger series of coal-measure than has hitherto been noticed, characterized by the freshwater limestone above alluded to.

2. The recurrence of these beds at Coalbrook Dale, over an older series of coal-measures which at one spot repose on mountain limestone, at other places either on the old red sandstone or on transition rocks.

3. The absence of these upper beds at the Titterston Clee hills, where the lower beds rest in two places on mountain limestone, but generally on old red sandstone, as they do invariably on the brown Clee hill, in the forest of Wyre and at Newent.

4. In some of the poor and ill-consolidated coal-beds, particularly in the upper part of the series, the characters of the fossil plants, both generic and specific, can be recognised in the coal itself.

5. The mountain limestone where it does occur in this part of the country is of inconsiderable thickness, and wedge-shaped, so that it shortly disappears entirely. Its absence, therefore, is not to be imputed to mighty convulsions, but to partial and scanty deposition in the first instance.

At Shaftoe, near Wallington, in Northumberland, Mr. Trevelyan* has observed among the constituents of the millstone grit, the lowest bed of the regular coal-measures, transparent fragments of garnet; they occur there rather abundantly. He has also remarked in other northern coal-fields small portions of hornblende in a similar situation.

The Rev. Mr. Williamson has directed attention to certain ravines in the Mendip hills, and other heights which bound the coal-field of Bristol. These ravines cross the ridges transversely so as to connect the opposite valleys, being occupied in part by horizontal beds of dolomitic conglomerate and lias; he infers that the fractures took place before these rocks were deposited, and that the bone-caves were formed at the same period.

Dr. Lloyd first observed fossil fishes in the old red sandstone. Mr. Murchison finds the observation true over a considerable extent of country; they belong chiefly to the genus Cephalaspis of Agassiz; they have also been described by Dr. Fleming, as having been met

* Lond. and Edinb. Phil. Mag. and Journ. of Science, vol. vi. p. 76, (1835.)

with in the old red sandstone of Forfarshire. They appear not to be diffused through the formation generally, but to be confined to the middle portion of it, the cornstone.

The nature of the pebbles imbedded in the Old Red Conglomerate varies according to its locality. In Scotland they are very frequently of gneiss, as is the case in the neighbourhood of Baden-Baden.

In an Account of the Trap Rocks of the Border Counties, and their effects on the Stratified Beds with which they are in contact, Mr. Murchison has separated the objects of his examination into three classes: 1. the Trap-Rocks which penetrate transition beds; 2. those which penetrate the old red sandstone; 3. those which penetrate the coal-measures. He refers the whole to igneous action, and considers them to be of the same age as the rocks with which they are respectively associated, rocks which he readily admits to be sedimentary, since, though composed of volcanic materials, they contain organic remains. The igneous action he conceives has taken place under water, and the finer volcanic ejections, arranged by Neptunian agency, have led to the formation of volcanic sandstones. His views upon this subject appear to be in exact accordance with those of Mr. de la Beche.

Treating of the relations between igneous and fossiliferous rocks, Mr. de la Beche observes*, that though frequently posterior, the former are in many cases contemporaneous with the strata in which they at present occur, appearing to have covered an inferior bed, and to have been subsequently covered themselves by a tranquil deposit of transported matter, as lava may flow over a sandy bottom and afterwards be covered up by sand or mud. Trappean rocks, he continues, are in various parts of Europe much associated with the lower parts of the grauwacké series, sometimes in a manner which leaves no doubt that some of them have not been included among the strata after their consolidation, while others have clearly forced a passage through the grauwacké and previously formed masses of trap. Beds of greenstone or porphyry, he says, sometimes fine off among the grauwacké strata, taking the character of an arenaceous deposit, as if such portions constituted a deposit of trappean ashes, thrown out at the same time that the trappean rock itself was produced. Brent Tor, north of Tavistock, remarkably exemplifies some of these appearances.

The researches of your Vice-President in the counties of Devon and Somerset have been carried on this year with increased energy. Of the eight sheets of the Ordnance Map upon which he has been engaged, four were published last spring, three others are complete, the eighth is nearly complete, and an explanatory memoir with sheets of sections applying to the whole are to be published before our next anniversary. Let us hope that this work so admirably begun may not be suffered to terminate here.

[To be continued.]

* Researches in Theoretical Geology, p. 384.

ZOOLOGICAL SOCIETY.

[Continued from vol. vi. p. 394.]

Dec. 9, 1834.—Specimens were exhibited of three species of the genus *Bulinus*, Lam., which were regarded by Mr. G. B. Sowerby as previously undescribed. He characterizes them as Bul. *leucostoma, badius*, and *bicolor*.

The specimens were brought to England by Mr. Miller, to whom the Society is indebted for their exhibition.

The reading was concluded of Mr. G. Bennett's paper on the *Ornithorhynchus*, an abstract of which was given in our Number for April last, Lond. and Edinb. Phil. Mag., vol. vi. p. 307.

December 23.—Drawings were exhibited of four *Fishes* of the River Quorra, made by Lieut. Allen, Corr. Memb. Z. S., from specimens obtained by him during his late voyage up that river into the interior of Africa. They exhibit the forms of *Lates*, Cuv.; *Mormyrus*, Ej.; *Sudis*, La Cép; and *Notopterus*, Ej.; and thus tend, in common with the specimens from the same expedition exhibited at the Meeting of the Society on June 10 (Lond. and Edinb. Phil. Mag. vol. v. p. 311), to illustrate the analogy borne by the *Fishes* of the rivers of Western Africa to those of the Nile.

A specimen was placed on the table of a *Toucan*, apparently hitherto undescribed, and forming part of the collection of N. C. Strickland, Esq., by whom it was communicated for exhibition.

Mr. Gould, at the request of the Chairman, pointed out its distinguishing characteristics. By its comparatively short bill, which is furrowed on the sides, and broad and flattened on the *culmen*, with the base of the under mandible extending obliquely beyond the line of the eye; by the shortness and roundness of its wings, of which the fourth quill-feather is the longest, the fifth, sixth, and seventh being nearly of the same length; and by the comparative shortness of the tail, which is less decidedly graduated than in the typical *Pteroglossi*; this bird agrees with the species described in Mr. Gould's 'Monograph of the *Ramphastidæ*,' as the *Pter. prasinus*, Licht., and *Pter. sulcatus*, Swains. With those species Mr. Gould proposes to associate it in a group, to be designated, on account of the grooved bills of the *Birds* comprised in it, *Aulacorhynchus*. From the other two species it is readily distinguishable by the white band nearly surrounding the base of its bill, and by the blood-red spot on the rump. The latter character affords the trivial name of the species, which may, for the present, be inserted in the account of the *Toucans* given by Mr. Gould at the Meeting of July 8, 1834, (vol. v. p. 383.) immediately before the *Pter. prasinus*, Licht., under the name of *Pter. hæmatopygus*. The precise part of South America in which this bird was captured has not been ascertained.

Col. Sykes, when reading to the Society, in 1832, (see vol. ii., p. 230.) his Catalogue of the Birds of Dukhun, not having exhibited the nest and eggs of the *Lonchura Cheet*, and of that species of *Tailor-bird* which he denominated *Orthotomus Bennettii*, brought them under the notice of the Society on the present occasion.

The nest of the *Lonchura Cheet* is a perfect hollow ball, made of a delicate *Agrostis*, with a lateral hole for the entrance of the birds.

It contained ten oblong minute white eggs, $\frac{13}{20}$ths of an inch long by $\frac{9}{20}$ths in diameter. It was found in the fork of a branch of the *Mimosa Arabica.*

The nest of the *Orthotomus Bennettii* was lodged in the cavity formed by sewing the edges of two leaves together: the nest itself also was attached to the leaves by threads passing through the leaf and the bottom of the nest, and there were appearances of the end of the thread being knotted outside. The nest is composed of very delicate fibres of *Indian Hemp* and grass. It contained two minute oblong crimson eggs, $\frac{3}{20}$ths of an inch long by $\frac{9}{20}$ths wide.

Col. Sykes also exhibited an egg of the *fluviatile Tortoise* of Dukhun, *Trionyx Indicus,* Gray. It is a perfect sphere, $1\frac{4}{10}$ inch in diameter: the calcareous shell is of a peculiar alabaster-like whiteness. He found seven eggs with shells in the oviducts, and twenty-seven without shells, nearly of the size of the preceding, in one specimen. He took occasion to mention that in the stomach and intestines of another specimen of *Trionyx,* he found not only the animals, but also angular fragments of considerable size of the shells of some scores of large *Uniones.*

A paper was read, entitled, "Description of some Species of *Chama*: by W. J. Broderip, Esq., Vice-President of the Geological and Zoological Societies, F.R.S., L.S., &c."

The author commences by remarking that the shells of the genus *Chama* appear to be subject to every change of shape and often of colour which the accidents of their locality may bring upon them, and that the distinction of the species must consequently be difficult, on account of their infinite variety. He then proceeds to describe those brought home by Mr. Cuming, and now in that gentleman's cabinet. The *Shells* referred to were exhibited in illustration of the characters and descriptions, under the following specific names: Chama *frondosa, pellucida, lobata, Pacifica, imbricata, producta, corrugata, echinata, spinosa,* and *sordida.*

A Note by Mr. George Bennett on the Nasal Gland of the *wandering Albatross, Diomedea exulans,* Linn., was read. It described in detail the gland situated in that bird above the orbit, as observed by the writer in 1832, and accorded with the account of it published by him in the Appendix to his 'Wanderings in New South Wales,' &c. It was illustrated by a drawing of his dissection of the head of an *Albatross,* made specially with the view of tracing the excretory duct of the gland, which he succeeded in doing for nearly two inches under the external plate of the upper mandible, in a direction towards the nostrils, but inclining slightly upwards, until he lost sight of it among the cellular substance of the bone. The writer notices the occurrence of a corresponding structure in other *Birds,* particularly among the *Natatores,* and refers to Müller for an account of the gland as it exists, in or near the orbit, in species of every order of *Aves.*

A specimen was exhibited of a *Kangaroo,* recently brought from New Holland, by Capt. Sir W. Edward Parry, R.N., and presented by him to the Society.

Mr. Bennett called the attention of the Meeting to it as representing a species not hitherto described, and distinguishable by its paler colour, which is generally of a slaty grey; by the whiteness of its tail throughout the greater part of the length of that organ; by the comparative length of the tail, which is here longer than the body, whereas in the ordinary *greater Kangaroo, Macropus major*, Shaw, it is shorter; by the comparative nakedness of the ears; by the great extent of the naked muzzle; and by a broad white stripe along each cheek. He stated it to be his intention to describe it in detail under the name of

Macropus Parryi. *Macr. rhinario lato; auriculis elongatis nudiusculis; caudâ corpore sublongiore, pilis rigidis brevibus incumbentibus vestitâ: notæo griseo; gastræo pallido; fasciâ genarum, caudâque pro maximâ parte, albis, hâc ad apicem nigrâ.*

Long. tot. a rostro ad caudæ apicem 5 ped. 4 poll.; *capitis*, 6 poll.; *auriculæ*, 4; *tarsi postici*, ad unguis longioris apicem, 10½; *caudæ*, 2 ped. 6 poll.

In a Note from Sir Edward Parry, which was read, it is stated that the animal in question is known to the natives in the neighbourhood of Port Stephens (lat. 32° S.) by the name of *Wōllăroo*. This individual had been in his possession in New South Wales for two years previously to his embarkation for England, and was allowed to range about at perfect liberty. It set out every night after dusk into the bush to feed, returning generally about two o'clock in the morning. In addition to what it obtained on these excursions, it ate meat, bread, vegetables, &c. Occasionally, but rarely, it ventured out in the daytime to a considerable distance, in which case it would sometimes be chased back by strange dogs: these, however, it always outstripped by its superior swiftness, until it placed itself under the protection of the dogs of the house. It died, from the effects of an accident, almost immediately after its arrival in England.

Detailed Notes of its dissection by Mr. Owen were read. The structure of its principal *viscera* corresponds in general with that of the same organs in the *greater Kangaroo*, but there are some differences observable in the anatomy of the two species. The puckering of the stomach, which is occasioned in *Macr. major* by three longitudinal bands, one extending on each side from the *œsophagus* along the lesser curvature, and the third passing along the line from which the great *epiploon* is continued to the spleen and transverse *colon*, depends in *Macr. Parryi* on the lateral bands alone, there being no mesial one. The different segments of the intestinal canal bear the same relative proportion to each other in both species; but the length of the several segments, and consequently of the whole canal, is less as compared with that of the body in *Parry's* than in the *greater Kangaroo*,—a fact which is in direct accordance with the more mixed nature of the food in the former. The spleen in *Macr. Parryi* was deeply notched at its free trenchant margin; in *Macr. major* it appears to be always entire. The mesial *cul-de-sac* of the *vagina* did not extend quite so far down in *Macr. Parryi*, as it does in the better-known species.

In the stomach were found two hair-balls of an oval shape, not rounded as they generally are in the *Ruminants,* which are most obnoxious to these formations. One of them was 3, and the other 2 inches in the long diameter. They were entirely composed of the hairs of the animal, matted together and agglutinated by the mucus of the stomach. Mr. Owen remarks on the interest which attaches to this resemblance to the *Ruminating* tribes, to which the *Kangaroos* make so near an approach in the complexity and magnitude of the stomach, and the simplicity of the *cæcum* and *colon.* He states that he has "more than once observed the act of rumination in the *Kangaroos* preserved in the Vivarium of the Society. It does not take place while they are recumbent, but when they are erect upon the tripod of the hinder legs and tail. The abdominal muscles are in violent action for a few minutes; the head is a little depressed; and then the cud is chewed by a quick rotatory motion of the jaws. This act was more commonly noticed after physic had been given to the animals, which we may suppose to have interrupted the healthy digestive processes: it by no means takes place with the same frequency and regularity as in the true *Ruminants.*"

January 13, 1835.—A specimen was exhibited of the *brush-tailed Kangaroo, Macropus penicillatus,* Gray, which had recently been presented to the Society by Captain Sir W. Edward Parry. Mr. Bennett called the attention of the Meeting to its peculiarities, and remarked on the great hairiness of the tail, and especially on its want of robustness at the base, as indicating probably the type of a new genus, to be removed from among the *Macropi* on account of the diminished power of an organ which is so exceedingly strong among the typical *Kangaroos* as to execute, during the act of slow progression and while resting, the office of a third leg. In connexion with this peculiarity of tail, Mr. Bennett pointed out also a difference in the form of the third, or extreme lateral, incisor, as compared with the corresponding tooth in *Macr. major,* Shaw; *crania* of the two animals being exhibited for that purpose. The third incisor in *Macr. penicillatus* is bilobed, and approaches somewhat to the character of the corresponding tooth in *Macr. Parryi,* Benn.

A note by Sir W. Edward Parry, which accompanied the specimen, was read. The animal appears to be procurable with difficulty, as this individual was "the only one of the kind ever seen by Sir E. Parry. It was shot among rocks near Liverpool Plains, New South Wales. As several of the same kind were seen together on more than one occasion, they appear to be gregarious. They seemed to prefer the neighbourhood of rocky ground, in which they had holes, to which, when hunted, they retreated. The first intimation received of these animals by Mr. Hall was, that monkeys were to be seen in a particular situation: and the manner in which they jumped about, when he first approached a number of them, left the same impression on his mind. They were so wild that he found it impossible, on his first attempt, to obtain a specimen; and one which he had wounded escaped into its hole. Some months afterwards, how-

ever, after remaining on the spot a whole night for the purpose, he succeeded in killing one towards daylight, which is the specimen now presented to the Society."

Mr. George Bennett stated that while in New South Wales he had heard of an animal called *Gúnar* by the natives, and found about the Beran Plains, which was described to him as in some degree resembling a *Kangaroo*, but differing from it in having a bushy tail, and in the form of the head, which was stated to resemble that of the *Hare*. He suggested the probability that the *Gúnar* and the *brush-tailed Kangaroo* might be specifically identical.

Extracts were read from a Letter addressed to the Secretary by M. Lesson, For. Memb. Z.S., and dated Rochefort, December 29, 1834. It was accompanied by the subjoined table of a distribution of the families of the *Acalepha*, Cuv., proposed by the writer.

ACALEPHA.

I. Without a central solid axis.	
A. Body simple, entire.	
1. Symmetrical, terminated at each pole by an opening.	1. Beroideæ.
2. Non-symmetrical: the upper pole disciform or umbrella-shaped, imperforate.	2. Medusæ.
B. Body multiple or aggregated.	
a. Homogeneous.	
3. Composed of two pieces adhering together, and capable of separation from each other.	3. Diphydes.
4. Composed of numerous pieces aggregated together.	4. Polytoma.
b. Heterogeneous.	
5. Animal furnished with appendages of different kinds.	
* Vesicle small, regular, placed at the summit of a kind of stalk furnished with lateral *ampullæ* and terminal suckers.	5. Physsophoræ.
** Vesicle large, irregular, without stalk or *ampullæ*, but having terminal suckers and cirriferous processes.	6. Physalia.
II. With a central cartilaginous axis.	
6. Body simple, with suckers and lateral *tentacula*.	
a. Body irregularly oblong, with a vertical *lamina* on its upper surface.	7. Velellæ.
b. Body discoid, flat above.	8. Porpitæ.

A letter was read, addressed to the Secretary by B. H. Hodgson, Esq., Corr. Memb. Z.S., and dated Nepâl, February 25, 1834. It

gave a systematic and technical account of the *Chiru Antelope, Antilope Hodgsonii,* Abel, in conformity with the latest and most complete information possessed by the writer, and communicated by him to the Society at its Meeting on July 22, 1834.

ASTRONOMICAL SOCIETY.

March 13.—The following communications were read :

I. Catalogue of 76 stars. By R. Snow, Esq.

This catalogue is an experiment towards determining how nearly the results of a small transit may be brought to accord with those of large observatories. The instruments employed were a 20-inch transit, on a cast-iron stand and a brickwork pier, and a sidereal chronometer beating half-seconds. When seven wires were not used (which was generally the case) those employed were reduced individually to the mean of the seven by a subsidiary table obtained from 10 transits of *Polaris,* and 20 of δ *Ursæ Minoris.*

Mr. Snow enters into the detail of his method of observing, and the results are as follow. Of the 76 stars, 67 are in the Greenwich catalogue of 1112 stars, of which 5 absolutely agree with the Greenwich observations, 41 differ by less than $0^s,1$, 17 by more than $0^s,1$ and less than $0^s,2$, and 4 by more than $0^s,2$; the greatest difference being $0^s,26$. The *plus* and *minus* errors are equally divided, the sum of 31 of the first being $2^s,95$, and of 31 of the second, $3^s,03$; giving, when signs of errors are taken into account, $-0^s,0012$ as the mean difference of the two catalogues.

II. Immersion of ζ *Libræ,* July 15, 1834,—of ϵ *Capricorni,* September 17, 1834,—and of ξ^1 *Arietis,* February 4, 1835, observed at Ashurst. By R. Snow, Esq,

III. Opposition of *Vesta,* in November 1834, observed at Edinburgh. By Mr. Henderson.

IV. Opposition of *Jupiter,* in November 1834, observed at Edinburgh. By Mr. Henderson.

V. Inferior conjunction of *Venus,* in December 1834, observed at Edinburgh. By Mr. Henderson.

VI. Opposition of *Mars,* in January 1835, observed at Edinburgh. By Mr. Henderson.

VII. Observations by the Transit and Mural Circle of *Mars* and stars in his parallel, made at Paramatta, from October to December 1832. By Mr. Dunlop. Communicated by Sir Thomas Brisbane.

VIII. Observed Right Ascensions and South Polar Distances of *Vesta* and stars in her parallel, with the transits of the stars on which the right ascensions depend; observed at Paramatta, in July 1833. By Mr. Dunlop. Communicated by Sir Thomas Brisbane.

IX. Observations of a Comet in 1833, and of another in 1834, observed at Paramatta by Mr. Dunlop; with Remarks by Mr. Henderson, in a letter to Sir Thomas Brisbane. Communicated by Sir Thomas Brisbane.

The comet of 1833 was very small, and was observed from September 30 to October 16. That of 1834 was observed from March

21 to April 14. On the latter, Mr. Dunlop remarks, (March 21,) that it resembled a small bright nebula, about $1\frac{1}{2}'$ diameter, with a very faint stream of light proceeding from the head, *at intervals*, exceedingly rare, and of a very pale bluish colour, remarkably different from that of the head. The first was observed with a parallel wire- and the second with a ring-micrometer.

The places of the comets, according to Mr. Henderson, are given in the Monthly Notices.

X. Observed Transits of the Moon and Moon-culminating Stars over the meridian of Edinburgh Observatory, in January and February 1835.

XI. Transits of the Moon with Moon-culminating Stars, observed at Cambridge Observatory in the months of January and February, 1835.

PROCEEDINGS AT THE FRIDAY-EVENING MEETINGS OF THE ROYAL INSTITUTION OF GREAT BRITAIN.

[Continued from vol. vi. p. 394.]

May 1.—Dr. Lardner on Halley's comet, and its reappearance in the autumn of this year.

May 8.—Professor Wheatstone on the construction and uses of speaking machines.

May 15.—Mr. Faraday on the condition and use of the tympanum of the ear.

May 22.—Mr. Brockedon on the storms which occurred on the Alps in August 1834.

May 29.—Mr. Davidson on Thebes.

June 5.—Mr. Cowper on engraving, carving, and sculpturing by machinery.

June 12.—Mr. Wilkinson on the manufacture and force of gunpowder.—Conclusion of the season.

CAMBRIDGE PHILOSOPHICAL SOCIETY, LENT TERM, 1835.

[Continued from vol. vi. p. 396.]

Monday (May 4th), the Rev. G. Peacock, the Treasurer, in the Chair. Prof. Airy gave an account of recent results obtained at the Observatory; namely, 1st. That the discrepancy of the observations of the obliquity of the ecliptic at the summer and winter solstices formerly noticed, had disappeared on using the refraction corresponding to a new barometer which stands 1-10th of an inch higher than the one formerly used. 2nd. That the mass of Jupiter, as determined by observations of the 4th satellite in 1834, is almost exactly the same as that obtained in 1832 and 1833, namely, 1-1048th of the sun's mass. 3rdly. That the time of rotation of Jupiter, as determined by a spot, is $9^h\ 55^m\ 21^s$: the spot from which this determination was obtained made 225 revolutions in 93 days.

Afterwards Mr. Whewell gave an account of the results of his examination of the tide observations made last June at the stations of the coast-guard service.

Monday evening (May 18th), Prof. Airy, V.P. in the chair. A paper

by Mr. A. Smith, of Trinity College, was read, containing a simple method of performing the eliminations by which we may obtain Fresnel's equation to the wave surface in biaxal crystals, according to the undulatory theory of light. Mr. Whewell read a letter from Prof. Schumacher in which it was stated that Messrs. Bier and Mödler have, by observations of two remarkable spots during several months, fixed the time of Jupiter's revolution at $9^h\ 55^m\ 26\frac{1}{2}^s$ being a longer time by $5\frac{1}{2}$ s. than that mentioned by Prof. Airy at the last meeting, as the result of his observations. It was also stated that M. Bessel had observed a long series of elongations of Jupiter's satellites, and that these give the mass of Jupiter nearly identical with that obtained by Prof. Airy. Mr. W. M. Fisher made further observations in confirmation of the views explained in his former communication respecting Tubercles.

Monday evening (June 18th), Dr. Clark, V.P. in the chair. Mr. Willis gave an account, illustrated by models, of the progress of architectural art in the vaulting of churches in the middle ages. He observed that the Romans had devised arrangements by which parallelograms of unequal sides could be covered with vaults, as, for instance, in the baths of Diocletian. In this case we have parts corresponding to the side aisles, buttresses, and clerestory windows of the churches of later times. But a great revolution took place in the decorative construction of such vaults when, instead of resting on their supports as a solid mass, the ribs alone were multiplied to receive the increased number of members of the vaulting and of the pier arches, so as to form clustered piers. Differences were noticed between the treatment of such piers in England and in other countries.

IX. *Intelligence and Miscellaneous Articles.*

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE: MEETING AT DUBLIN IN AUGUST NEXT.

THE next Meeting of THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE will take place in Dublin, and will occupy the week, commencing on Monday, August 10th. It is anticipated that this meeting will present as many objects of interest to the friends and cultivators of science, and will be as numerously attended as those which have been held in previous years at Oxford, Cambridge, and Edinburgh.

The following is extracted from the circular addressed to the members by Dr. Hamilton and the Rev. H. Lloyd, Secretaries for Dublin.

"Dublin, July 1st, 1835.—It is requested that Members who may have any papers, or other communications, to lay before the Association, will state, before the end of July, their general nature and probable extent, in letters addressed as follows:

To the Provisional Secretary of the [Mathematical or Chemical, or other] *Section, Royal Irish Academy, Dublin.*

Unless this precaution be attended to, great inconvenience must arise,

and valuable papers may be rejected at the last, through confusion and press of business.

"It is also very desirable that the gentlemen who propose to attend should signify their intention early, to the nearest local Treasurer, or to one of the Secretaries for Dublin.

"Information for members, on their arrival, will be given at the Examination Hall of Trinity College, at which place there will be an attendance of proper persons for that purpose during the week of the Meeting and the preceding week.

"The following persons are entitled to be Members of the Association on paying the annual subscription of 1*l*., or 5*l*., which is the composition in lieu of annual payments.

"1. Fellows and members of chartered societies in the British Empire, publishing Transactions. 2. Office-bearers, and members of the councils or managing committees of Philosophical Institutions. 3. Members of any Philosophical Institution recommended by its council or managing committee.

"Other members are also elected by the general committee of the Association, or in the interval of the Meetings by the Council."

ON THE FREQUENT DEFICIENCY OF THE UNGUEAL PHALANX IN THE HALLUX OF THE ORANG OUTANG.

In the first portion of the Memoir of the late Sir T. Stamford Raffles, the publication of which was commenced, some years since, in the Zoological Journal, an intention is expressed (Zool. Journ., vol. iii. p. 41 *note*,) of noticing, in the sequel, the history of the animal or animals to which the names of *Orang Outang* and *Pongo* had been applied by naturalists. In pursuance of this intention (which I designed to fulfill when reciting the contributions to Zoological science of that lamented statesman and philosopher, who had himself called attention to the subject by a remark in his Catalogue of a Zoological collection formed in Sumatra), I examined some of the relations of naturalists and travellers respecting the *Orang Outang* and its affinities, collected references to others, and entered, to a certain extent, into the discussion of the results derivable from them. The Memoir itself, however, having been afterwards discontinued, the materials thus prepared, like the sketch of the history of the Papuans, (but which I have since inserted in Lond. and Edinb. Phil. Mag., vol. i. p. 466,) and notices of other subjects also connected with the natural history of the Indian Archipelago, have remained unpublished in my possession.

My attention has been recalled to this subject, after the lapse of nearly seven years, by Mr. Owen's recent investigation of the comparative osteology of the *Orang Outang* and *Chimpanzee*, towards the further prosecution of which, in relation particularly to certain points in the natural history of the former species of *Simia,* I have had the pleasure of supplying Mr. Owen with some references.

In the abstract of Mr. Owen's paper given in our last Number (and in the Proceedings of the Zoological Society, No. xxvii.), it is remarked that "The peculiarity of the structure of the *hallux* first

noticed by Camper, in seven out of eight *Orangs* observed by him, viz. its possessing no ungueal *phalanx*, and consequently no nail, loses much of its importance as a specific character from the fact that the individual dissected at the Society's Museum a few years since had very perfect, but small, black nails, and two *phalanges*, and that the same number of *phalanges* exist in the natural skeleton of Lord Amherst's *Orang* in the Museum of the College of Surgeons." (Lond. and Edinb. Phil. Mag. vol. vi. p. 466—467).

During my search for the references which Mr. Owen had requested, I found that, from a comparison of the various statements then extant respecting the absence or presence of the nail of the *hallux* in specimens of the *Orang*, I had arrived, in 1828, at the conclusion, that its deficiency was not connected with any distinction of species among the more anthropoid *Simiæ*. As this particular subject has again been brought under the attention of zoologists by Mr. Owen's inquiries, it seems proper now to make public, as follows, the remarks upon it which I originally drew up, exactly as they were prepared for insertion in the Memoir of Sir Stamford Raffles. They were intended, I may add, to form a note, to be attached to a synopsis of the additions to Zoology contained in Sir Stamford's "Descriptive Catalogue of a Zoological Collection made in the Island of Sumatra and its vicinity," which was published, in 1821, in the Transactions of the Linnean Society.

"Sir Stamford states, when describing the Sumatran *Simiæ*, [in the Catalogue just referred to, Trans. Linn. Soc., vol. xiii. p. 241,] that in a living specimen of the Orang Outang, *Simia Satyrus*, Linn., sent from Borneo to the Menagerie at Calcutta in 1819, the nail of the thumb was wanting on the 'hind-feet.' The present imperfect and confused state of the information possessed by naturalists respecting the animal or animals to which the names of Orang Outang and Pongo have been applied, renders it interesting to note this circumstance, since the specimen sent to Calcutta has, no doubt, been preserved for future examination. Camper and Linnæus, it will be remembered, regarded this deficiency of the thumb-nails in the hinder hands as a general character of the Orang Outangs, the truth of which statement, however, has been denied by Cuvier, (*Règne Animal*, tome i. [edit. 1817] p. 103 [edit. 1829, p. 88])."

"In an article on the Orang Outang of Borneo, by Mr. J. Grant, just published [July, 1828] in No. xvii. of the Edinburgh Journal of Science, we are informed that the great toes (hinder thumbs) of the Sumatran animal so called, described by the late Dr. Abel, have well-defined nails, resembling in shape and size those on the other 'toes' (hinder fingers). In the Pongo or Orang Outang of Borneo described by Wurmb, as appears from the same article, 'the nails of the great toes' were much smaller and shorter than the rest. In the hinder hands of an Orang Outang, brought from Borneo a few years since, which are deposited in the collection of the Trinity-House, Hull, and have lately been described by Dr. Harwood, 'the thumbs are each destitute of a nail, but they have a hardened protuberance in its place.' (See Trans. Linn. Soc., vol. xv. p. 472).

Finally, the Bornean animal described by Mr. Grant, in the article just quoted, possesses nails on the hinder thumbs; while Mr. Montgomerie, who had obtained this specimen from Borneo, states that it is the only one he has observed to be possessed of the nail on the great toe; but ‘that this is the only particular in which he differs in the slightest degree’ from the other Bornean Orang Outangs which he has seen, amounting in number, as appears in the sequel, to at least ten or twelve.

“Whether two or more species of *Simia* have been confounded together under the appellations of Orang Outang and Pongo, or whether the discrepancies in the accounts of naturalists have arisen from the examination of individuals of different ages, it would be premature to attempt to decide. But the legitimate inference from the facts above stated appears to be, that at all events the want of the hinder thumb-nails is a deficiency of frequent occurrence in the animal or animals in question, unconnected with any important variations in external character or in organization, and unconnected, therefore, with specific distinctions.”

I shall not again enter upon the consideration of the subject, further than to observe, that the cursory review which I have thus been led to take of it, at the present time, induces me to modify the opinion which I formerly entertained, as expressed in the concluding paragraph of the above note. Since it appears by uniting the statements of Camper, (Phil. Trans. vol. lxix. p. 145, 146,) Mr. Owen, and Dr. Jeffries, (Phil. Mag., vol. lxvii. p. 182,) with those which I had originally examined, that out of 26 or 28 individuals of the animal in question, 18 or 20 were deficient in the nail of the *hallux*, there seems reason to regard it as a distinction of some importance, though still not amounting to a specific character. With reference also to Mr. Owen's idea that certain characters are indicative of varieties of the *Orang* inhabiting distinct localities, I may remark that it would seem from the various statements which have been cited, that all the individuals so characterized have been obtained from Borneo, although that island has also furnished a few others exhibiting the normal structure; among 23 or 25 individuals from that locality 18 or 20 not possessing the nail of the *hallux*. If anything further can be deduced from the facts now before us, it is the probability that there exist in Borneo two varieties of the *Orang Outang*, one of them having the normal, and the other the said abnormal structure of the *hallux*. Should this prove to be the fact, it will remain to be determined whether the former Bornean variety is not identical with the Sumatran; and whether the varieties thus characterized are also distinguished by any peculiarities of the *cranium*, such as those which have been detected by Mr. Owen on comparing two different skulls of the *Pongo*, or adult *Orang Outang* of Borneo.

London Institution, June 23, 1835. E. W. B.

ON OIL OF CINNAMON, &c.

MM. Dumas and Peligot observe that there occur in commerce several varieties of cinnamon and two of the oil, which are easily

distinguished. That which comes from China is of a reddish brown colour, and its smell is disagreeable. The oil from Ceylon is sugary, and has a sweet smell; it is better than the former.

Not being able to rely upon the oil of cinnamon procured in commerce, MM. Dumas and Peligot prepared it by distilling it from the bark. In order to obtain the pure oil, the bark must be bruised, and left twelve hours digesting in a saturated solution of salt, and then subjected to rapid distillation over a naked fire. The water is milky, and allows the oil to deposit; and the water suffered to remain in contact with the air becomes filled with lamellar and acicular crystals.

When oil of cinnamon is treated with concentrated nitric acid, it almost immediately concretes, and forms a true crystallized salt, in which the oil serves as a base. This characteristic phænomenon is very imperfectly produced in the oil of commerce from China and Ceylon, requiring from eight to twelve hours for producing the effect; and while the pure oil is converted into a hard, friable, colourless, crystalline mass, the oils of commerce always give a butyraceous product, the crystals of which are evidently mixed with a deep-coloured oleaginous substance, the nature of which is unknown. Oil of cinnamon combines with dry gaseous muriatic acid. The purest becomes of a deep green tint; it forms a crystallizable product with ammonia, unchangeable when exposed to the air.

Oxygen gas is rapidly absorbed by oil of cinnamon, especially when it is humid; it forms in this way a new acid, which the authors call *cinnamonic acid*; it appears to be similar to that produced in old oil of cinnamon, or in cinnamon water exposed to the air.

When oil of cinnamon is treated with hot nitric acid, a strong smell of bitter almonds is produced, and when the action of the acid is over, a great quantity of benzoic acid is found in the residue. If oil of cinnamon is boiled with a solution of chloride of lime, there is also found a quantity of benzoic acid, or rather of benzoate of lime. The action of chlorine on this oil presents some phænomena of great interest; the chlorine acts at first in forming a chloride of benzoyle; but when its action, aided by heat, is over, a very stable crystalline compound is procured, in constitution approximating to a chlorate.

Oil of cinnamon appears as a substance which acts the part of a base. It combines with acids; and MM. Dumas and Peligot do not think that the action of ammonia upon it is of such a nature as to modify the conclusions drawn from the action of acids. The oil prepared as above gave by analysis:

C^{36}	1377·3	82·1
H^{16}	100·0	5·9
O^{2}	200·0	12·0
	1677·3	100·0

Journal de Chimie Médicale, June 1, p. 217.

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ATOMIC WEIGHT OF ALUMINUM.

In Silliman's Journal for January last is a paper by Mr. Mather,

Instructor of Mineralogy, &c., on "Chloride of Aluminium and its Analysis." This paper extends over nearly twelve octavo pages, and is one of the most perfect specimens of the *lengthy* ever inflicted on the chemical world; about 2000 figures and symbols are employed, and the results are commonly carried out to eight decimal places. The conclusions arrived at are, that the atomic weight of aluminum, according to the experiments detailed, is 0·87920118, or 1·31880177, accordingly as we consider alumina a proto- or sesquioxide; and that the hydrated chloride of aluminum is composed of

Chlorine	1 atom	= 25·52
Aluminum	1 atom	= 6·40
Water	10 atoms	= 68·08
		100·00

ON VEINS OF CRYSTALLIZED CARBONATE OF LIME IN FOSSIL WOOD. BY EDWARD CHARLESWORTH, ESQ.

To the Editors of the Philosophical Magazine and Journal of Science.

GENTLEMEN,

In the examination of some English specimens of fossil wood, from several different localities, my attention has been drawn to a circumstance which I do not remember to have seen noticed, although I think it can hardly have escaped the observation of others; I allude to regularly formed veins of crystallized carbonate of lime, which intersect the wood in a transverse direction, completely separating it into a number of unequal lengths, and that without producing the slightest displacement or alteration in the position of those fibres which are immediately in contact with the intersecting substance. My observation was first directed to this fact in some of the petrified wood so frequently picked up on the Norfolk coast, and to which a comparatively recent date is generally assigned: I believe, however, that the most frequent instances occur in the secondary formations.

A short time since a dealer brought me from Maidstone a block of greensand containing a beautiful piece of dicotyledonous wood, which illustrates in a very interesting manner the circumstance in question. The specimen is about eight inches long, and is intersected by five or six bands of carbonate of lime, which pass entirely through it, and are then attached to the walls of the surrounding cavity. These transverse veins are again crossed by longitudinal ones, so that a number of cells are formed, presenting an appearance similar to that which is constantly seen in the nodules of limestone from the London and Kimmeridge clays, generally known by the name of Septaria.

The portions of wood thus inclosed do not appear to have suffered any derangement of structure, the continuity of the fibres being simply interrupted by the extraneous substance. I am certainly inclined to think that this effect was produced during the progress of petrifaction, and that it is not the result of previous or subsequent infiltration. As any facts connected with that process are interesting, I am induced to bring these few remarks before your notice, thinking that they

may perhaps lead to a further investigation of the subject. I ought perhaps to remark that in those specimens where the circumstance above mentioned has taken place, carbonate of lime is, in all probability, the principal lapidifying agent.

I have observed something similar, but not so distinctly shown, in wood that has become completely silicified.

I remain, Gentlemen, yours, &c.

Guy's Hospital, March 23, 1835. EDWARD CHARLESWORTH.

PHÆNOMENON RESEMBLING THE MIRAGE, SEEN IN THE REGENT'S PARK.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Perhaps the following occurrence is not so frequently observed in England as to make a notice of it unworthy of publication in your Magazine.

Upon walking along the newly constructed promenade in the Regent's Park, ascending the first hill from the Zoological Gardens, as soon as the eye is raised a little above the level of the highest part, the appearance of a sheet of water crossing the path between this hill and the next elevation is visible, with the reversed images of distant persons and objects reflected in it. Of course this phænomenon is most conspicuous on a very warm day: it reminds the spectator of similar occurrences in sandy deserts.

Camden Town, June 23, 1835. I am, &c. J. DE C. SOWERBY.

EXPERIMENTS TO ASCERTAIN THE EXISTENCE OF LEAD IN THE ATMOSPHERE OF A WHITE-LEAD MANUFACTORY. BY MR. ARTHUR DUNN.

To the Editors of the Philosophical Magazine and Journal of Science.

GENTLEMEN,

Having witnessed at my manufactory the frightful effects of white-lead on the workmen employed, I was anxious to determine if it was possible for lead to exist in the atmosphere, and through that medium be absorbed into the system by the action of the lungs. For this purpose I made the following experiment, which certainly is important to the manufacturer, as it points out a serious evil to be guarded against. I shall now merely confine myself to the results obtained, and leave to some of your more scientific friends any theoretical reasoning or practical hints the experiment may suggest, provided you consider it worthy to occupy a space in your valuable Magazine, and remain, Gentlemen, yours, &c.

Chemical Works, City Road,
January 19th, 1835.
ARTHUR DUNN.

An evaporating dish, containing about 28 lbs. of moist carbonate of lead, was placed in a sand-bath, and heated to about the same temperature as the drying stove commonly used, never exceeding 150° Fahr.: over this was fixed, at the distance of from eight to twelve

inches, a pair of common bellows, with a glass tube attached to the pipe, which pipe was introduced into a green glass bottle containing 12 ounces of distilled water, acidulated with 2 drachms of nitric acid. The apparatus being thus arranged, the bellows were set in action, by which means the atmosphere, loaded with the moisture from the lead, was made to pass in a continued current through the liquid: this was continued for six hours. The whole was then transferred into a platina dish and evaporated to perfect dryness. The residue was dissolved in one ounce of distilled water with two drops of nitric acid to insure the solution of the lead should any be present. A current of sulphuretted hydrogen was next passed through the solution, which immediately gave a minute dark precipitate; this being collected on a filter and washed, was transferred to a watch-glass, and treated in the usual manner with nitric acid to decompose the sulphuret, which gave on the application of hydriodate of potash the most unequivocal proof of the presence of lead.

Another experiment was conducted at the same time with similar vessels in the same room, but the current of air was not passed through the liquid. This on the application of sulphuretted hydrogen gave not the least indication of lead, but, on evaporating the whole to dryness and treating the residue in the manner before described with hydriodate of potash, the slightest possible trace of the yellow iodide of lead was perceptible. The nitric acid and distilled water were separately tested with great care, but were found perfectly free from lead, so that no doubt the trace of lead must have been absorbed from the atmosphere, as the bottle containing it stood beside the one through which the current of air was passed. I ought to have mentioned before that the temperature of the laboratory during the experiment was from 70° to 80° Fahr., and that the door was kept closely shut that the air might be loaded as much as possible with the vapour.

DECOMPOSITION OF CYANURET OF MERCURY BY IRON.

A question having arisen as to the propriety of heating prussian blue and peroxide of mercury in an iron vessel for preparing cyanuret of mercury, on account of the supposed decomposition of the salt by the iron, M. Guibourt, in order to decide the question, dissolved some cyanuret of mercury in distilled water, and placed a plate of iron in the solution. In a few hours spots of oxide appeared on the iron, and the solution when agitated had an ochreous tint. The next day the oxidation of the iron had made great progress, and the liquor had acquired a sensible odour of hydrocyanic acid. The action continued during four days: the iron was then almost entirely oxidized on the surface, corroded in several spots, which had a bluish black colour, indicating the formation of prussian blue. The liquor had a strong smell of hydrocyanic acid, and contained much iron in suspension; and at the lower part of the plate of iron, and the bottom of the bottle, globules of mercury were observed in considerable quantity.

The liquor was shaken and thrown upon a filter: the globules remained at the bottom of the bottle; the oxide of iron was retained by the filter, and the colourless liquor passed through it. It did not act

upon syrup of violets; potash did not evolve any ammonia from it, and on being evaporated to dryness a considerable quantity of cyanuret of mercury remained undecomposed. The ochreous matter remaining on the filter was treated with water acidulated with muriatic acid; the peroxide of iron having been dissolved in this manner, there remained an insoluble matter of a dark greenish blue colour, which was a mixture of prussian blue and very finely divided mercury. It results from this experiment, that when iron is left in contact with a solution of cyanuret of mercury, water is decomposed; its oxygen combines with the iron, and its hydrogen with the cyanogen, while the mercury is reduced to the metallic state. This action, however, as Scheele has remarked, is slow, but it is instantaneous if a little sulphuric acid be added.

Although an earthen vessel is, in the opinion of M. Guibourt, preferable to an iron one in preparing the cyanuret of mercury, yet it may be made in the latter without any great inconvenience, on account of the crust of oxide with which iron vessels are commonly covered when used in laboratories.—*Journal de Chimie Médicale*, April 1835.

SULPHURET OF ZINC AND IRON.

M. Bouis of Perpignan has analysed a double sulphuret of zinc and iron which occurs at Cabrera on the right of the road from Arles à Prats to Mello. This sulphuret has a brown colour; its fresh fracture is scaly, with small interlacing laminæ of a brownish grey metallic lustre. Its specific gravity varied from 3·2 to 3·6 when cellular, and was 3·85 when compact. Its powder was brown, effervesced slightly with an hepatic odour when put into diluted sulphuric acid. It yielded by analysis:

Sulphuret of zinc	67·00
Sulphuret of iron	19·86
Oxide of zinc	0·16
Peroxide of iron	4·00
Silica	4·00
Water and carbonic acid ..	4·20
Loss..................	0·78
	100·00

The pure mineral is therefore composed of

3 atoms of sulphuret of zinc	77·045
1 atom of sulphuret of iron	22·955
	100·000

Ibid.

Scientific Books nearly ready for Publication.

The Second Edition of the First and Second Reports of the British Association for the Advancement of Science.

Transactions of the Linnæan Society, Vol. XVII. Part ii.

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London, and by Mr. VEALL, at Boston.

Days of Month. 1835.	Barometer.			Thermometer.			Wind.		Rain.	
	London.		Boston.	London.		Bost.	Lond.	Bost.	Lond.	Bost.
	Max.	Min.	$8\frac{1}{2}$ A.M.	Max.	Min.	$8\frac{1}{2}$ A.M.				
May 1	29·673	29·658	29·13	57	36	50	sw.	calm	0·05	...
2	29·669	29·632	29·22	53	39	47·5	w.	calm	·08	0·66
3	29·850	29·776	29·30	56	40	47	sw.	calm	...	...
4	30·044	29·890	29·34	61	38	52	N.	calm	...	...
☽ 5	30·113	29·987	29·51	64	47	55	s.	calm	·12	...
6	29·886	29·832	29·26	65	42	55	sw.	calm	...	...
7	30·091	29·952	29·25	62	37	56	w.	calm	...	·20
8	30·136	30·059	29·47	70	52	57	s.	calm	...	...
9	30·010	29·835	29·37	70	48	61	sw.	NW.	...	...
10	29·881	29·784	29·22	66	41	59	sw.	w.	...	...
11	29·974	29·843	29·40	63	50	55	sw.	w.	...	·13
○ 12	29·841	29·646	29·30	63	46	54	w.	calm	·20	...
13	29·616	29·487	29·04	64	43	53	sw.	w.	1·10	·21
14	29·642	29·495	29·08	59	44	44	NE.	E.	·66	·23
15	29·747	29·712	29·35	62	41	44	s.	E.	·06	·30
16	29·827	29·768	29·26	64	41	56	sw.	calm	·04	·29
17	30·037	29·950	29·38	74	39	58	sw.	w.	...	...
18	29·906	29·877	29·30	76	46	63·5	s.	E.	...	...
☾ 19	29·876	29·806	29·38	72	47	61	E.	E.	...	...
20	30·112	29·935	29·50	60	48	52	E.	calm	·03	...
21	30·196	30·180	29·72	63	37	53·5	N.	calm	...	...
22	30·171	30·088	29·72	71	48	56	w.	calm	·06	...
23	30·116	30·111	29·60	72	52	60	w.	calm	...	...
24	30·127	29·961	29·57	73	53	61	w.	NW.	...	...
25	29·885	29·642	29·27	66	45	59	w.	w.	·02	...
26	29·546	29·493	28·99	59	40	52	w.	NW.	·22	...
● 27	29·891	29·682	29·26	65	38	55·5	w.	calm	·44	...
28	30·023	29·952	29·55	63	39	57	E.	E.	·04	...
29	30·138	30·057	29·66	60	37	57	E.	E.	...	...
30	30·104	30·082	29·70	60	36	52	N.	E.	...	...
31	30·046	29·941	29·61	66	45	56	NW.	calm	·26	·08
	30·196	29·487	29·37	76	36	54·8			3·38	2·10

Remarks.

London.—May 1. Rain: fine. 2. Slight haze: heavy showers: fine. 3, 4. Fine: cloudy. 5. Very fine. 6. Rain: fine. 7, 8. Hazy: fine. 9—11. Fine. 12. Overcast. 13. Cloudy: fine: heavy rain at night. 14. Rain. 15. Fine: heavy showers. 16—19. Fine. 20. Hazy: slight rain. 21. Fine: cold at night. 22. Fine: rain. 23, 24. Very fine. 25. Overcast: fine. 26. Rain. 27. Cloudy and fine: rain at night. 28. Hazy: showers. 29, 30. Fine, but cold. 31. Fine: rain at night.

Boston.—May 1. Cloudy: rain A.M. 2, 3. Cloudy. 4, 5. Fine. 6. Cloudy: rain P.M. 7, 8. Fine. 9. Cloudy. 10. Cloudy: rain P.M. 11, 12. Cloudy. 13. Cloudy: rain A.M. 14. Rain. 15. Rain: rain P.M. 16—19. Fine. 20, 21. Cloudy. 22—25. Fine. 26, 27. Cloudy. 28, 29. Fine. 30. Cloudy. N.B. Three degrees colder than 31st December last. 31. Fine: rain P.M.

THE

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[THIRD SERIES.]

AUGUST 1835.

X. *Observations on the Crag-formation and its Organic Remains; with a View to establish a Division of the Tertiary Strata overlying the London Clay in Suffolk.* By EDWARD CHARLESWORTH, *Esq.**

A KNOWLEDGE of the comparative distribution of organic remains throughout the various strata of the globe, a close attention to the different circumstances under which such remains have been deposited, and an intimate acquaintance with the resemblance borne by them to existing genera and species, are points now generally regarded as being of the highest importance in connexion with geological deductions. I shall not, therefore, deem any apology necessary for offering the following observations in reference to a subject which, though affording ample scope for more extended investigation, has till within a late period been very generally neglected.

It is not a little singular that while the tertiary formations of Great Britain and the Continent have recently excited so large a share of attention, a portion of our own island, occupied by so interesting a deposit as the Crag, should have remained comparatively unexplored.

With the exception of one or two notices in the Geological Transactions, we had little correct information on the subject till the appearance of Mr. R. C. Taylor's work on the Geology of Norfolk, in 1827, subsequently to which Mr. Samuel Woodward published his Outlines of the Geology of the

* Communicated by the Author. This paper was read before the Geological Society, May 27, 1835.

same district. These works, however, refer particularly to the crag of Norfolk, which it will be seen does not present nearly so extensive a series of fossils, or perhaps such interesting geological features, as the same formation in the adjoining county. I must also mention the observations made by Mr. Lyell during a survey of the Norfolk and Suffolk coasts in the summer of 1831, and which are so ably detailed in his Principles of Geology.

The latest published account of the crag is in Professor Phillips's Guide to Geology*, which I quote as illustrating the notions commonly entertained respecting the character of this formation.

"CRAG-FORMATION. *Mineral character.* — It resembles almost exactly a shingle or pebble beach with layers of sand and shells, being composed of pebbles of various sorts, rolled and worn fish-teeth and bones; a few bones of quadrupeds, also worn; many shells, sometimes worn, sometimes not; parts of Crustacea, Polypifers, &c. The whole has an ochraceous aspect from the admixture of oxide of iron. In one situation a coralline limestone of the same age occurs, and includes several of the same shells."

This extract gives a very good idea of the appearances often presented by the crag, and particularly by those portions which have hitherto fallen under the inspection of geologists: but there is a very important part of the formation to which the description is by no means applicable, whether we regard its general mineralogical characters, or the apparent circumstances under which its organic remains have been deposited.

The crag of Suffolk rests on a substratum of clay, which, though often differing in character from the blue clay of London and Hampshire, probably belongs to that formation, as numerous teeth of fish and occasionally large *Nautili* are found in it†.

The most favourable situation for viewing the crag as it usually occurs is along the south-eastern coast of Essex and Suffolk, where its bright sands, mixed with prodigious quantities of worn and broken testaceous remains, are seen topping the cliff; the beds varying in thickness from five to twenty feet. Here the crag rests unconformably upon the London clay, without any material alteration in its character or appearance. The continuity of the stratum is interrupted by

* A Guide to Geology, by John Phillips, F.R.S. &c. Second edition, 1835. p. 104.

† A splendid Nautilus, found beneath the crag, adorns the chimney-piece of a cottager by the fort at Walton. I have also seen several at Walton-on-the-Naze in Essex.

several estuaries, which having extended a few miles into the interior, terminate at the junction of the crag with the common diluvium of the country. The banks of these estuaries are generally low, or slope so gradually as not to furnish advantageous sections of the district through which they pass. The crag, however, is exposed in several places at a village called Ramsholt, on the eastern bank of the Deben; and here an entirely new feature presents itself.

Beneath the common stratum of shells and pebbles is a bed of sand devoid of that peculiar deep tinge which so generally accompanies the crag-formation: this bed is in contact with the London clay, and contains a great assemblage of organic remains; but these exhibit so novel a character, and the circumstances under which they were deposited were evidently of so different a nature from those of the superior stratum, as at once to strike the attention of the most casual observer. My object in the present paper will be principally to establish the following position:—That the circumstances attending the formation, and the difference in the organic remains of these beds, indicate their having been deposited at distinct periods.

The term *Red Crag* may be appropriately applied to those beds which constitute the upper and most extensive part of the formation, while I propose to designate the lower as the *Coralline Crag*, for reasons which will in the sequel appear sufficiently obvious.

It is now some years since I first observed at Tattingstone, a village situate on the western boundary of the crag, and between the rivers Orwell and Stour, that a quarry had been worked to a greater depth than usual, when, instead of reaching the London clay, a stratum had been exposed containing several shells which were then new to me. Many of these had the corresponding valves in contact, a circumstance of unusual occurrence; and the deposit altogether presented an appearance differing in many respects from that of any part of the crag with which I had been previously acquainted. I subsequently met with the same shells, associated with similar peculiarities of deposition, in Sudbourne Park, (the occasional residence of the Marquis of Hertford,) about twenty miles eastward of Tattingstone. It was not, however, until the discovery of a corresponding stratum at Ramsholt, where the organic remains exceeded in number and excelled in preservation those of any other locality, that my attention was particularly drawn to the consideration of the subject.

The relative depth of the strata in the different localities was as follows:

Tattingstone. Alluvium and gravel, 4 feet.

Horizontal layers of sand, with few shells, 15 feet.

Layers of sand with numerous shells, inclined at an angle of about 45°, showing a beautiful illustration of false stratification, 4 feet.

Coralline crag, 6 feet. This stratum had been exposed for about seventy yards: in attempting to dig through it, I was stopped by the appearance of water at the further depth of two feet.

Ramsholt. Red crag, 4 feet. Coralline crag, 7 feet.

The two beds may be traced by the side of the Deben for a considerable distance; but owing to the quantity of brushwood and alluvial soil with which its banks are covered, their position can only be advantageously seen in one spot, where an excavation has been made.

Sudbourne Park. Here the red crag is absent: the inferior stratum has been extensively, though but superficially, quarried, its greatest depth being about twelve feet.

The first striking peculiarity attending the coralline crag in either of the above localities is its general uniformity of character, having none of those variations in colour and stratification which are so constantly met with in the upper beds. Upon a closer examination we find it composed of siliceous sand, with a large proportion of calcareous matter, sometimes assuming a moist and slightly adhesive character, but never blending with the dry loose soil of the accompanying strata, a line of demarcation being always distinctly apparent*. We see here none of those effects which are ascribed to the operation of tides and currents; there are no layers of sand alternating with worn shells and pebbles: but the appearances are those that would naturally be the result of a deposit going forward during a tranquil state of the surrounding medium. The remains of *Testacea* are extremely abundant, but furnish no indications of having been subjected to the action of waves on a pebbly shore: on the contrary, they are often little inferior, in state of preservation, to those of the *calcaire grossier*. Shells of the same species are often found so grouped together that they could not possibly have been removed from their original locality; this is particularly the case with a large undescribed species of *Balanus*, occurring at Ramsholt sometimes in clusters of twenty or thirty. The moveable valves still remaining in the interior of the shell. The following are

* The separation between the red and the coralline crag is as distinctly evident as that between the crag and the London clay; there is nothing like a gradual transition from one to the other. This is an important fact, as it either indicates an intermediate period, or a sudden change in the operation of those causes which were in action when the crag was deposited.

among some of the most abundant genera: *Pectunculus*, *Cyprina*, *Cytherea*, *Astarte*, *Venericardia*, *Ostrea*, and *Pecten*. In most instances these shells can be distinguished from those of the red crag by a difference in their appearance, not depending upon any peculiarity in the shells themselves, but owing to the circumstance of their not having been subjected to attrition, and to the soil in which they are imbedded containing a smaller proportion of the oxide or hydrate of iron.

We might reasonably conclude that a deposit presenting the appearances just described, would be more favourable for the preservation of the organic remains peculiar to the crag æra, than those parts of the formation which had previously been explored, and we accordingly find that not only are the shells generally in a more perfect condition, but that numerous species abounding here have not been discovered in the superincumbent strata. A far more important circumstance is that many of the most common and characteristic shells of the red crag have not been obtained from the coralline; in illustration of which may be mentioned the total absence of *Buccina* and *Murices*, two genera which, perhaps, of all others are distributed in the greatest profusion throughout the whole of the upper beds.

I cannot, however, proceed further, without alluding to the very valuable information connected with the organic remains of this stratum which I have received from Searles Wood, Esq. (of Hasketon near Woodbridge), who by the most active and persevering exertions has more than trebled the amount of *Testacea* usually assigned to the crag; and his cabinet exhibits so extensive a series of these fossils, as will probably for a long period far exceed that of any other collector. Having communicated to Mr. Wood my intention of drawing up the present observations, he very kindly furnished me with an account of his collection. The following is an extract from a letter dated March 1835.

"With respect to the two distinct formations in Suffolk, you will be able to point out how far you think they extend. I have never observed the lower bed to the westward of the river Deben: the locality you speak of at Tattingstone, I must have overlooked. That there are two distinct beds in Suffolk, is an observation that has long been made; I have heard my friend the Rev. G. R. Leathes speak of a distinct stratum of shells and corals at Ramsholt, nearly twenty years ago, and he then remarked how much they resembled those of the crag. In the lower bed are found a few teeth of cartilaginous fish, and some vertebræ of the spinous species. The more characteristic shells of the crag I have not yet observed, but from

the known habit of particular species congregating in distinct localities, those portions of the stratum may have been destroyed, in which these shells, perhaps, constituted the principal part of its organic remains.

"I am inclined to think the whole of the upper stratum has been produced from the ruin of the lower. I herewith give you a summary of my cabinet, as far as relates to the *Testacea*: the classes *Polypi*, *Radiaria*, and *Crustacea*, of which numerous remains are found, cannot at present be enumerated. Many of the shells from the upper crag have long been figured by Sowerby: I have collected 235 species; two thirds of these are also met with in the lower. The following is the joint production of the two strata; and from the labour I have bestowed upon the subject, unless some new localities be discovered, I think the list will not for some years be much extended.

Annulata	13	species.
Cirripeda	11	
Conchifera	189	
Mollusca	257	

"Making a total of 450 species.

"The last number includes 50 species of minute cephalopodous mollusca of the order *Foraminifera*, D'Orbigny, which exclusively belong to the lower stratum, and which I think I am entitled to the claim of bringing into notice*."

It appears by this statement that of 450 species collected by Mr. Wood, about 80 are peculiar to the red crag, upwards of 200 peculiar to the coralline, and the remaining 150 common to the two formations.

One or two writers in alluding to the crag have observed, that it assumes a remarkable change in character at Aldborough, the eastern boundary of the formation. Mr. R. C. Taylor particularly remarks this circumstance, describing this part of the stratum as a "soft porous rock, mixed with interesting varieties of corals and sponges." Aldborough, however, does not afford so favourable a field for the researches of the geologist as Sudbourne and Orford, two parishes on the estate of the Marquis of Hertford, and about six miles from the former locality. Here the quarries are very numerous, but, with the exception of that described in a former part of this paper, they afford only a small proportion of tes-

* This discovery of the minute cephalopodous mollusca, by Mr. Wood, is a most interesting one. Mr. Wetherell of Highgate (whose extensive and valuable collection of fossils is well known,) showed me several species which he had procured from the London clay.

taceous remains. The deficiency is, however, fully compensated by the abundance of corals, many of which are extremely beautiful, and distinct from recent species. These corals sometimes occur in a loose sandy grit, from which they are readily detached; but it frequently happens that *the stratum is almost wholly constituted by them*, numerous species and genera indiscriminately growing upon one another, the interstices being filled with sand, dead portions of coral, comminuted shells, and other extraneous substances, all of which have become cemented together, occasionally forming a rock sufficiently compact for the purposes of building. It is well known that a process analogous to this is going forward in the coral reefs of the present day, and to which also many coralline limestones probably owe their origin.

It sometimes happens when the sides of a quarry have long been exposed to the action of the rain, that the sand and other extraneous matters are washed away, leaving the corals exposed in a curious and beautiful manner.

From my examination of this part of the crag, I think there is every reason to conclude that it was formed under similar circumstances, and is contemporaneous with that inferior stratum which I have described as seen in other parts of the county. Not only do many of the corals so characteristic of the Orford beds abound in some parts of the stratum at Ramsholt, but in both instances we have the same peculiarities in the character of a deposition, associated with the presence of certain shells and the absence of others. The excavations in these two parishes are of frequent occurrence, but I have not yet discovered even a fragment of the *Fusus contrarius*, or a single specimen of *Murex* or *Buccinum*. In the absence of all negative proofs, this correspondence in so striking a peculiarity would have considerable weight in establishing some relation between the different localities; but when we see that the deposit in both instances differs from the rest of the crag in having been entirely of subaqueous origin, and that in both this change of character is accompanied by the same peculiar Zoophytes and *Testacea*, we may fairly conclude that even should the Ramsholt and Orford beds prove not to be continuous, they are at least contemporaneous*.

* I have dwelt rather strongly upon the connexion between the crag of Ramsholt and that of Orford and Sudbourne, because in the latter places the inferior stratum is not hidden by the upper beds, and consequently affords much greater facilities for its examination.

The division which I have made of the crag will tend very much to simplify those variations which have been so frequently observed in its mineralogical character. The upper beds almost invariably present the appear-

An interesting circumstance connected with the organic productions of the coralline crag, is the presence of a larger number of the *Echinidæ* than generally appear to have existed during the formation of tertiary strata. I have not yet ascertained the precise amount of species, but they include the following genera, *Echinus*, *Cidaris**, *Scutella*, and *Fibularia*; the last also occurs in the upper strata.

The teeth of fish are distributed in considerable abundance throughout the greater part of the red crag in Suffolk: most of them exactly resemble the teeth so frequently met with in the London clay of the Isle of Sheppey and other places, and which probably belong to fish allied to the genus *Squalus*.

In the crag these teeth have undergone some peculiar chemical change, which has given them a dark colour, and rendered them extremely hard; they are generally much worn, and exhibit a brilliant polish, which sometimes extends over that portion formerly surrounded by the alveolar cavity. Mr. Wood remarks, that he has found a few teeth of cartilaginous fish in the coralline crag: I have only seen two specimens from it, but they had the enamel perfect, and presented that delicate pearly lustre, shown by the teeth which occur in the tertiary beds of Malta, and from which locality I should certainly have supposed them to have been obtained, if I had not been previously acquainted with their history.

It is usually considered that the remains of land animals are more or less blended with the marine productions of the crag. Flattened portions of bone of a very compact structure, highly polished by attrition, and having their specific gravity much increased, are often picked up on those parts of the coast where the crag has been exposed†: with this exception, the instances of Mammalian remains occurring in the Suffolk beds are extremely rare.

ance described by Professor Phillips, while the deviations in the lower depend upon the unequal distribution of shells and corals, either of which predominating, occasions an alteration in the nature of the deposit.

Mr. Taylor says that in Essex the crag shells sometimes occur in "strong blue clay." I think that he must have derived this idea from Smith, who in his 'Strata Identified', uses precisely the same expression, probably confounding the crag with another formation.

* The occurrence of this genus is stated on the authority of Mr. Taylor, who in describing some of the corals from the crag, mentions a *Cidaris* as being attached to one of them. See Loudon's Magazine of Natural History for 1830, page 274.

† These fragments have lost all character of the animal, or even particular bone, to which they belong. They may possibly be exterior portions of the femur of the elephant.

Bones of elephants and other quadrupeds are much more frequently associated with the shells of the crag in Norfolk; but in that county the formation in many places exhibits such irregularities, and is sometimes so mingled with immense accumulations of sand and gravel, that it becomes almost impossible to distinguish the specific crag deposit from the accompanying diluvial strata. At Cromer patches of crag are seen covered by cliffs of such materials two hundred feet in thickness; and in the neighbourhood of Norwich, beds of shells were reached after boring through eighty or a hundred feet of diluvium *.

To what extent, then, the remains of herbivorous animals occasionally occurring in the crag may be considered contemporaneous with it or as belonging to a more recent period †, is certainly a question of considerable interest, but upon which the limits of my present communication will not allow me to enlarge; I shall, therefore, only observe that the coralline strata have not furnished the slightest vestige of the *Mammalia*, a circumstance of considerable importance in connexion with the view which I have taken respecting the distinct period to which I attribute their formation.

We now come to the most interesting part of the inquiry,—the comparative age of the red and coralline crag, and their geological relation to each other. A due consideration of the preceding facts will, I think, naturally lead to one of the following conclusions: either that at the time when the eastern parts of Norfolk and Suffolk were covered by the ocean, deposits were going forward, which, within a very small compass, present in their general character and organic remains differences of a most unaccountable and extraordinary nature; or that, in accordance with my previous proposition, the tertiary formations overlying the London clay in Suffolk have been deposited at distinct periods.

There would be nothing inconsistent with our knowledge of the habits of marine animals, in supposing, that owing to a variation in depth or other circumstances, many species of *Testacea* would congregate in particuliar spots; and that in

* Mr. R. C. Taylor on the Geology of East Norfolk.

† Some of these bones have undergone the same peculiar change as the teeth of fish; which would lead us to consider that they have been imbedded in the crag during a similar period, and are therefore of the same or perhaps greater antiquity than that formation.

Since the above observations were laid before the Geological Society, Dr. Buckland has very kindly forwarded to me some of the proof-sheets of his forthcoming Bridgewater Treatise; in the first volume of which, among his remarks upon the existence of *Mammalia* during the tertiary epoch, he mentions, as one instance, the bones found in the crag of Norfolk.

some places deposits would be forming, which might afterwards, from an alteration in the force or direction of currents, have materials of a very different nature carried down upon them. On a subsequent examination of these we should not only find alterations in their character and stratification, but probably corresponding variations in the accompanying shells. Upon this principle we could imagine that a change might be exhibited in the character of a deposit, as extensive and as uniform in its nature as that presented by the tertiary strata now under consideration. There are, however, some circumstances, the occurrence of which would prevent our adopting this theory, should it be suggested, as explanatory of the phænomena observed in the instance before us.

The red crag affords decisive evidence of having been a gradual deposit formed by successive accumulations of marine exuviæ, which were not brought from a distant part by the operation of a powerful current, but belonged to the natural inhabitants of those localities, which, owing to the subsequent retreat of the ocean, are now rendered accessible. Among the most abundant of these reliquiæ were numerous species of the genera *Murex* and *Buccinum*, of which no traces are discoverable in the coralline crag, although its sands were admirably calculated for the preservation of such *Testacea* as existed at the time of its formation*.

If we refer to the localities in which the inferior beds have been exposed, we find one situate in the most central part of the crag, while the others form the eastern and western boundaries. On examining these spots, there is nothing which would lead us to imagine that the lower stratum is a mere local deposit; on the contrary, there is every indication of its extending beneath the superincumbent beds, but owing to their general horizontal position, and the slight depth to which artificial excavations are usually carried, there will probably be considerable difficulty in ascertaining the precise limits of the district it occupies.

The hundred and fifty species common to the red and coralline crag may, perhaps, be brought forward as a conclusive proof of their intimate relation to each other: one consideration, however, must not be lost sight of, namely, whether some of the shells now occurring in the red crag may not originally have belonged to the coralline. In some parts of Nor-

* It should be remembered that the *Murices* and *Buccina* are by no means the only absentees in the coralline crag. The number of species deficient is altogether eighty, including the *Tellinæ*, *Cypræœ*, &c., while at the same time we find an addition of more than 200 new species.

folk it is not unusual to find the *Terebratula plicatilis* and other fossils of the chalk associated with those of the crag; now our extensive acquaintance with organic remains enables us to ascertain that these have been derived from a secondary formation. If, then, the action of the waters which deposited the red crag was sufficient to break up the chalk, and mingle its shells with species then living, what would be the natural result of the same force exerted upon the sandy beds of the coralline crag, abounding with *Testacea*, and offering comparatively no resistance to the abrading influence of tides and currents? We can hardly imagine any limitation of the extent to which the organic remains in the one would not become blended and interspersed with those in the other. The very recent appearance presented by many of the *Turbines* and *Buccina*, found only in the upper beds, when compared with species common to both, is certainly in favour of this view of the subject. I would, however, bring it forward rather as a suggestion than as considering it necessary to advance such a theory in support of the opinions which I have advocated respecting the separation of the tertiary beds in Suffolk*.

Should the facts which have now been adduced furnish sufficient data for arriving at that conclusion which it was my principal object to establish, we shall in the next place be led

* If the German Ocean were now to overflow the eastern counties of England, no one for a moment would think of asserting that a deposit left by it would be contemporaneous with the crag; and yet the connexion as shown by the shells would be just as great as in the instance before us. Of 111 species from the red crag, M. Deshayes identifies 44 with those now inhabiting the neighbouring seas. Of 370 species found in the coralline, Mr. Wood identifies 150 with those in the overlying deposit. The proportion in both instances would be very nearly the same, about 40 per cent. We can, therefore, entirely reject the idea of one deposit having derived some of its shells from the other, and yet a wide interval may have elapsed between the formation of the two.

There will, however, be no reason for adopting one theory to the entire exclusion of the other. Both may be taken into consideration, as accounting for the presence of the same species in different deposits. We see a similar occurrence, though not to so great an extent, in the subdivisions of the oolitic series.

Many parts of the red crag were extremely favourable for the preservation of shells, as is shown by the perfect condition of the *Cardium Parkinsoni* and *Mytilus antiquorum*, both particularly fragile species; and yet the whole line of coast from Walton-on-the-Naze in Essex, to Orford in Suffolk, with the numerous inland quarries, have furnished only 235 species, while the 370 from the coralline were procured by Mr. Wood exclusively from two spots in the neighbourhood of Ramsholt. This great difference in the amount of species would appear quite anomalous if we regard them as belonging to the same period; but the consideration that those in the coralline crag were deposited by an ocean inhabited by a larger number of *Testacea*, at once accounts for the circumstance.

to inquire what was the probable extent of the period which intervened between the formation of the coralline and the deposition of the red crag.

In the divisions now so generally adopted of the tertiary formations, the crag has been placed among the older Pliocene; I need hardly remark, that these divisions are founded upon the relative proportion of fossil shells which agree with existing species. The series of fossils from the crag examined by M. Deshayes amounted to only 111, and of these 66 were pronounced by him to be extinct. I much regret that at present no examination of this kind has been made of the newly discovered shells; they will probably by found to include a larger proportion of extinct species, and perhaps to an amount that would indicate their belonging to a period of higher antiquity than the Pliocene*.

In some parts of Suffolk, shells are found in the alluvial soil identical with those now inhabiting the German Ocean, and which have no connexion with the crag; these are probably contemporaneous with the brick earth of the Nar, a deposit in Norfolk evidently of recent origin. It is therefore of the utmost importance, in the examination of tertiary shells from these districts, to obtain accurate information of the precise localities in which they were procured.

Mr. Woodward describes the Norfolk crag commencing at Weybourne and Cromer, and running in a narrow band across the eastern part of the county. There is one circumstance that appears to me particularly worthy of attention connected with the amount of species obtained from this part of the formation:

As nothing analogous to the coralline crag has been observed in Norfolk, the deposit may there be looked upon as agreeing with the red crag of the adjoining county. On looking over the list of shells obtained by Mr. Woodward from the neighbourhood of Norwich, I find it contains only 80 species, while the upper or corresponding beds in Suffolk have furnished 235: this difference we should not have anticipated. If, however, the sandy strata of the coralline crag formed in Suffolk a bed for that ocean which in Norfolk rested on the chalk, the apparent inconsistency may be easily

* A comparison of Mr. Wood's collection with a recent series of British shells must be looked forward to with the greatest interest. With regard to the age of the crag, the question should first be settled, whether or not the two deposits indicate different periods. If the organic remains of each are considered separately, there will be some difficulty in determining the age of the upper one, unless the suggestions I have thrown out respecting some of its shells be overruled.

reconciled. Of the 235 species obtained by Mr. Wood from the red crag of Suffolk, only one third were peculiar to it, the rest being common to it and the coralline, and, as has been shown, might, perhaps, have belonged to the latter; if, then, these be deducted, the remainder, exclusively belonging to the red crag, will coincide in number with those obtained from Norfolk by Mr. Woodward.

It may be said that the announcement of a tertiary deposit differing essentially from any hitherto observed in this country, involves such important considerations, that it would be premature to form any positive inferences respecting its origin, until the subject shall have become one of more general geological investigation. It is true that the attention of geologists is now for the first time directed to the facts brought forward in this paper; but it must not be forgotten that these statements have been the result of careful observation during a considerable period of time. The splendid collection in the possession of Mr. Wood has been obtained after several years spent in the most indefatigable research, and the results he has arrived at regarding the numerical proportion of species in the two formations are not likely at present to receive any material alteration.

Should subsequent investigation of the crag overthrow the distinctions which I have attempted to establish, the newly described localities will at any rate offer a rich and interesting field for the labours of the oryctologist. The species already obtained from these spots nearly double the gross amount of our tertiary shells, and when we consider that they have been procured by the exertions of one individual only, we may conclude that at no very distant period the tertiary productions of our own country will equal in number those of the Paris basin*.

In conclusion I would only remark, that although considerable labour has been expended upon those parts of the coralline crag which are so particularly rich in the remains of *Testacea*, but little attention, comparatively speaking, has been paid to that portion of this formation which exhibits at its extremity so extensive and interesting an assemblage of Zoophytes. The specimens already procured from this district furnish sufficient evidence of the great abundance and

* The tertiary formations in France have furnished nearly 1200 species of *Testacea*. Those of the English crag may be fairly estimated at 500, as Mr. Wood in his list does not include all the Norfolk species. If to these latter we add those of the British Eocene period, the amount will not be greatly inferior to the French series.

variety in which the remains of this class have there been deposited. Hitherto the researches in this branch of the fossil productions of the crag have been extremely limited; but I cannot help expressing a hope that the great additions now made to our tertiary shells may be the means of drawing the attention of geologists to those parts of the formation which, though not containing the remains of *Testacea* in the same profusion as the stratum at Ramsholt*, present fossils of a no less interesting nature, and which would amply repay whatever time and labour might he devoted to their investigation.

13, Devonshire Square.

XI. *On the Importance of Studying and Preserving the Languages spoken by Uncivilized Nations, with the view of elucidating the Physical History of Man.* *By* Dr. Hodgkin.

[Continued from p. 36, and concluded.]

If the views of Dr. Von Martius are gloomy and calculated to throw discouragement in the way of philological and other researches into the origin and history of the American nations, those of Dr. Lang, which are diametrically opposed to them, may be thought to err on the other side.

Dr. Lang has been induced to believe that America derived its inhabitants from Asia through the Polynesian Islands. He appears successfully to get over the difficulty which would probably be the first to suggest itself in the way of this opinion, namely, that the course which he contends for would be opposed to the direction of wind and current. He brings forward the evidence of several distinguished navigators to prove that westerly winds and currents are of sufficient frequency and duration to allow of canoes and other vessels being carried by them from the Polynesian Islands to the western coast of America. This difficulty being set aside, the Doctor supports the probability of his hypothesis by setting forth various points on which the American nations exhibit a resemblance or affinity to the Polynesian Islanders. Even the languages of America, which are so numerous and so peculiar, instead of presenting an insuperable difficulty, have furnished Dr. Lang with a few arguments in his favour. Many of the

* The coralline crag at Ramsholt has been so undermined, that it is now very difficult of access. In the adjoining parish, Sutton, there is a quarry on the farm of Mr. Colchester of Ipswich, who politely affords every facility to those who are desirous of visiting it.

names of rivers, creeks, districts, towns, persons, and natural objects bear a decidedly Polynesian character; and some words bear this resemblance in signification also. He refers to the idea maintained by De Zuniga that the Philippine Islands were peopled from America, since the features of resemblance pointed out by him tend to strengthen the supposition of a remote connexion between the Americans and these islanders, whatever may have been the direction in which the communication took place. De Zuniga, in comparing the language of the Philippine Islands to some of those which are spoken by American Indians, says that they are strikingly conformable in their character and structure. Dr. Lang maintains the correctness of this assertion, notwithstanding its contradiction by Marsden. He notices Capt. Basil Hall's observation that the Indians of Acapulco bear a resemblance to the Malays. The advances towards civilization which had been made by the natives of America before the arrival of the Spaniards appear to have been of Polynesian character, as exhibited in their workmanship, their fortifications, temples, and images. Dr. Lang particularly compares the Mexican pyramids with one of a similar construction, 270 feet long, in Atehuru, which has unfortunately been destroyed. He notices also a similarity in their mode of worship and holding councils, in their superstitions with respect to the Taboo, and in the wide diffusion of cannibalism, which he conceives to have been maintained as an occasional rite, derived by the Americans from their Polynesian forefathers when the accidental plea of necessity no longer existed. I shall conclude this brief sketch of Dr. Lang's views with the following quotations from his work:

"It is doubtless impossible to fix the date of the original discovery of America with any degree of precision. Still, however, we may come within a moderate distance of the truth even on this dark subject. There is evidence to guide the judicious inquirer, scanty in its amount, doubtless, but definite in its announcements, and just as little likely to mislead as the records of ancient eclipses.

"1. The sources of this evidence to which we are naturally directed is the Polynesian language. In tracing the affinities of the Malayan and Polynesian tongues, I have already remarked that there are two epochs in the history of the former to which our attention ought to be especially turned. The first of these is the epoch of the Sanscrit, the second the epoch of the Arabic, infusion.

"Of the Arabic, or more recent infusion, the Polynesian language exhibits no trace whatever. We are therefore war-

ranted to conclude that the stream of emigration had ceased to flow from the Indian Archipelago towards the continent of America long before the æra of Mahomet, or the rise and prevalence of the Saracen power.

"Of the Sanscrit, or more ancient infusion, which has even changed the aspect and character of the ancient Malayan language, its Polynesian sister, or rather daughter, exhibits no tincture whatever. It follows, therefore, that the stream of emigration, which was destined to people the South Sea Islands and the continent of America, must have been flowing from the Indian Archipelago towards that distant continent long before the ancient Sanscrit language was spoken in the Indian isles. But that venerable language, like the Latin and Greek tongues in Europe, has been a dead language in India for many centuries. It must have been a living language at a period when a portion of its substance was imbedded into the Malayan tongue; a period, we have reason to believe, long anterior to the Christian æra. But before that period had arrived, the forefathers of the present Polynesians must have quitted the Indian Archipelago, and individuals of their number may perhaps have reached the far-distant American land.

"2. The religion of the Polynesians and the Indo-Americans indicates, in like manner, a remote antiquity. The idea that God is a spirit invisible to man, is still common to both of these numerous divisions of the human family.

"3. These indications of remote antiquity are borne out and corroborated in a remarkable manner by the style and character of those remains of ancient Polynesian, as well as of ancient Indo-American architecture, which have hitherto excited the wonder and mocked the ingenuity of the ablest speculators. These remains consist chiefly of the ruins of ancient temples, pyramids, and tumuli; the chief and the most remarkable characteristics of which are, the magnitude of their dimensions and the massiveness of their architecture compared with those of the ephemeral erections of modern times, and especially with those of the erections of the more recent aboriginal inhabitants of America and of the South Sea Islands. Now, it appears to me, that just as an architect who surveys the ruin of some ancient building for the first time, can at once tell the age or period to which its erection is to be assigned merely from the style of its architecture, and can pronounce it unhesitatingly either a Celtic, or a Saxon, or a Norman erection, there is a sort of internal evidence afforded by these most interesting remains of Polynesian and Indo-American civilization, which can enable an attentive ob-

server to ascertain, with a tolerable degree of precision, the age or period in the past history of man, to which their erection may be referred. In short, I conceive that the ruins in question afford us a means of ascertaining the period at which the forefathers of the modern Polynesian and Indo-American races originally took their departure from the Indian Archipelago." (Dr. Lang's Origin and Migrations of the Polynesian Nation, p. 203-4.)

The views of Dr. Lang, although founded on observation and supported by several independent facts, and also possessing in my opinion a great degree of probability, can only be regarded as an hypothetical solution of the mystery which involves the history and languages of the races to which it refers. It must be remembered that it is opposed to the views of that great Polynesian scholar W. Marsden, and is yet more decidedly at variance with the opinion of the learned author of an article on the Oceanic languages in the Foreign Quarterly Review. It is the opinion of that author that the various insular languages, as well as the continental, are in most cases distinct and indigenous, and that the numerous coincidences which are met with, and which he is compelled to admit as evidence of a common connecting cause, are the result of infusions from one common source into preexisting languages. To support this view the reviewer has recourse to the hypothesis that there had, at some former period, existed somewhere in the Indian Archipelago one of those independent foci of civilization which he calls in to his aid for the solution of several of the difficulties with which the subject abounds. I must confess that it seems to me much more probable that the languages spoken in the Oceanic Islands, whatever may have been their origin, have been introduced into the different islands very much in the same state in which we find them: this idea seems not only more consistent with the general similarity which prevails amongst the inhabitants of those islands with respect to peculiarities of race, manners, and religion, but also with the prevailing character of the languages themselves. Moreover, there is a want of simplicity in the conjecture that these islands have each received for themselves, by infusion from a common source, those words on which the most striking similarity depends; it seems to involve the necessity of numerous accidents, precisely similar to each other, having happened to each of these inhabited islands. Difficulties of this kind are sufficient to show that we are still greatly in want of the data from which a solution can be drawn.

It is of the utmost importance that those who observe, describe, and collect these data should give the naked but com-

plete truth, unbiassed by the opinion to which they may incline, although it may be perfectly allowable to them to contemplate, from time to time, the fragments which they may have brought together, since such surveys are not only pleasing in themselves but may be the means of pointing the way to new researches. Thus, I had no sooner read the view exhibited by Dr. Lang than my attention was awakened to what may be merely an accidental coincidence, but which nevertheless suggests an inquiry which may bring to light some curious facts. In the last number of the Asiatic Society's Journal there is an interesting abstract of a work by a Chinese antiquary on the ancient vases of his country. These vases, which appear to be as highly prized by the virtuosi of China, as the Grecian, Sicilian, and Etruscan vases are by the collectors of Europe, are calculated, like them, to afford a few important rays of historical light; in fact it appears that they afford data which are even more valuable, since they preserve inscriptions which are still legible, and may themselves be referred with greater certainty to particular ages. In looking at the sketches of some of these vases to which the greatest antiquity is ascribed, I was forcibly struck with a similarity between the designs with which they are ornamented and the carvings upon some implements brought from the South Sea Islands. One of the points of resemblance consisted in the mode in which uncouth representations of the human countenance are repeated upon the same article. I am well aware that there is no necessity to call in the assistance of remote connexion between the artists producing these works, in the fact of their both rudely attempting to imitate the same forms when the originals were equally present to both. It is the similarity in the mode in which they each deviate from nature which is, at least, remarkable. The attention which this casual observation excited, has induced me to look with greater interest at the variously carved and ornamented implements brought from the South Seas and preserved in cabinets, and I have observed that besides the characteristic and often beautiful patterns which these carvings exhibit, we may occasionally find the representations of human and other figures which, notwithstanding the rudeness of the outline, appear to have been destined to tell some story,—that in fact they are a kind of hieroglyphic.

Although the circumstances which these symbols are meant to record must now be lost in oblivion, it is by no means impossible that they may lead to the detection of a system for the expression of ideas, the investigation of which may be a useful adjunct to our inquiries respecting the oral languages of

these islands; at least in the total absence of alphabetic writing these imperfect attempts must not be wholly lost sight of.

With respect to the second part of Dr. Lang's views, namely, that America was peopled by the Polynesian race, it must I conceive be admitted, that although the Doctor has found more plausible reasons in its favour than might have been anticipated, and has also more successfully met objections which at first might be regarded as insuperable, there is, nevertheless, too great a chasm in the proof, both as respects the transport of individuals to the continent of America from the distant islands, and still more with respect to the connexion to be found between the very numerous American languages, and the very few or perhaps even the single one belonging to the Polynesian Islands, for us to receive his views in this respect as anything more than a conjecture which he has ingeniously rendered in a good degree probable. There is unquestionably a similarity between the form of the head which prevails amongst the inhabitants of the Pacific Islands and that exhibited by the skulls of Peruvians and other South Americans. This interesting fact had often forcibly attracted my attention long before I had any idea of Dr. Lang's views. Heads, of the form here alluded to, do not however exhibit the universal character of the American heads. Skulls have been found in ancient places of sepulture presenting so very remarkable a form that one or more distinguished naturalists have regarded it as an irrefragable proof that the skulls in question belonged to a totally distinct branch of the human race. I must confess that my views are decidedly at variance with the views of those naturalists, and avow my conviction that there is very adequate internal evidence that this peculiarity of form is the result of artificial causes applied to modify the shape. The position of the *foramen magnum* as compared with the facial angle, the lateral distortion of the head, and the situation of the greater part of the cerebral mass, go far to support my assertion. It is further to be observed that heads having somewhat of this remarkable form are found in company with other heads in which no distortion has taken place, but which, on the contrary, exhibit the character before alluded to as resembling that of the South Sea Islanders, whilst at the same time there are other reasons for inferring that the distorted and undistorted heads belong to the same race. Recent skulls exhibiting precisely the same character as to form, but with respect to which there is not only stronger internal evidence of distortion, but also the well-ascertained particulars as to the mode in which this distortion is produced, are met with over a wide extent of country quite in the North of North

America. That the Caribs, living at a great distance in another direction, procured a somewhat similar distortion by analogous means, is a well-known fact. We have therefore evidence of the wide extension of a similar custom whether a similarity of race be admitted or not.

If, however, we can find reasons in favour of the probability of America having in part at least received its population from the islands of the Pacific, we must not lose sight of those reasons which with at least equal probability give support to the idea that America was peopled from Asia from the north-west: the near approach of these continents in that direction, the traditions which still existed amongst the Mexicans when Europeans first became acquainted with them, and which Clavigero and Humboldt have so successfully rescued from oblivion, the very striking similarities between the calendar of the Mexicans and that of some Asiatic nations which Humboldt has pointed out, afford arguments in favour of this side of the question which are generally known and admitted. My friend Joseph Hawse, who has paid great attention to the languages spoken by the North American Indians, informs me that he has observed points of resemblance between these languages and those which are spoken by the Tartar tribes. This, however, is a subject on which much further philological research is required before we can rest satisfied with any conclusions which may be drawn. I must further remark, that if in the observation which I have made respecting a resemblance between the form of the head in some of the South Sea Islands and that of some races which either exist at present or have existed in America, more especially towards the western coast of South America, we find an argument in favour of the Polynesian descent of Americans, there is also evidence of a similar character in favour of an Asiatic origin by a north-western route. Through the kindness of my friend Captain Chapman, the museum of Guy's Hospital is possessed of casts of skulls which were taken from very ancient tumuli which existed not far from the Falls of Niagara, which tumuli are considered to have been the work of a race which inhabited that district prior to the Indian race with which we are at present acquainted. These skulls bear a most striking resemblance to those of the Esquimaux, which are known to exhibit the character of the Mongolian rather than of the Malay race. In the same tumulus with these skulls there were found various implements, such as bracelets of copper, breastplates of shell, and pieces of wampum, and, what is most remarkable, a large shell truncated so as to serve as a trumpet, which has been asserted to be an Asiatic and not an American production. It would appear

from these facts, that though we are only acquainted with the mere existence of this race by its very scanty relics, it has nevertheless transmitted to us historical evidence of a very strong and interesting character. Whilst their wampum is altogether American, and connects them with the present races, who only know them by obscure tradition, the form of their heads and their Asiatic shell no less strongly connect them with Asia.

If many of the languages which we have been considering are threatened with absolute extinction, together with the feeble families of the human race by whom they are spoken, in a way which has probably never been the case at any former period of the world, there likewise exists at the present time a greater amount of means which might be rendered availing for the preservation and investigation of these languages, together with the traditions, manners, and superstitions which it is essential to know, if successful and satisfactory attempts are to be made to elucidate those obscure portions of the history of mankind; a portion of history, be it remembered, which is intimately connected with the interests of religion, notwithstanding the barren aspect which it at first seems to present.

At no former period were there so many and so great facilities for sending to almost every part of the globe. The way seems opening even across the arid deserts and pestilential swamps of Africa, and the hitherto inaccessible regions of that continent are traversed by our enterprising countrymen, whom, on the other hand,

> " Non Boreæ finitimum latus
> Duratæque solo nives abigunt."

At the same time the cultivators of every branch of science are united into societies to extend an interest and to spread information on subjects connected with their respective researches. The progress which has been made in the physical geography of the globe attests how much geologists and mineralogists, botanists and zoologists, have done for their respective sciences. The geography of plants may now be taught to every child. The same means and facilities which have produced so many valuable results might be turned with equal success to the advancement of our knowledge of the geography of man; and I cannot help flattering myself that our Association, small as it is, may most essentially contribute to the attainment of this most important object: in the first place, by formally addressing those bodies and associations which are already in the possession of what may be regarded as the machinery essential to the prompt diffusion of information and inquiry. A formal appeal coming from a society would have much more weight than the influence which any single individual could possess.

With this view I conceive it might be well for us to draw up a collection of queries to be forwarded to the societies in question, accompanied with a short memorial urging the importance of the subject and soliciting cooperation. The Royal Society, the Geographical, the Zoological, the Linnæan, and perhaps the Society of Antiquaries, might be memorialized for this purpose, and I am inclined to believe that the officers of the Admiralty would be far from unwilling, if properly appealed to, to give most important aid in such an investigation. There are other bodies which I have not yet mentioned, but which in the assistance they may afford us are inferior to none; I allude to the different Missionary Societies. Some of these have far more extensive relations with some parts of the globe, and the feeble races inhabiting them, than any other bodies or associations. The zeal and devotion of their missionaries, which prompt them not merely to visit but to reside amongst the uncivilized or half-civilized families of the human species, would enable them to collect more extensive and more accurate information than can be collected by the traveller transiently passing through the district, whatever may be the superiority of his talents and acquirements. We are already indebted to some of those missionaries for most important and valuable information of the very kind which it is so desirable to collect. The name of Ellis, which must be familiar to every one, will sufficiently illustrate this remark. But what missionaries have done in this respect has been rather the fruit of their own good sense and individual interest than the result of a general purpose, or of instructions furnished to them by the Boards which have sent them forth.

The character of some of their exertions is thus described in an article on Kay's Caffraria, in the North American Quarterly Review.

"The attempt which the missionaries had made to translate special parts of the sacred volume into the Caffer language, manifests, in our judgement, more zeal than discretion. According to our author, their plan has been to translate a passage from the English version into 'barbarous Dutch,' and then to express it in the Caffer language as dictated by an ignorant interpreter. Till some better method than this is adopted many persons will distrust the benefit of the translation, since it is probable that error or nonsense will take the place of truth. However this may be, many converts having been made amongst the Caffers, and considerable inquiry excited amongst the people generally, the period is probably not far distant when Christianity, in some shape or other, will be openly professed by the whole nation. A translation of the

Bible may then be undertaken with a better prospect of fidelity, as the children of the missionaries, who will have learned the native language, or the Caffer children who have been instructed in the English, will be exempt from the inconvenience now experienced by the missionaries."

We cannot hope to draw sound and correct philological inductions from data thus obtained.

Many instances might be adduced to show that the zeal of the missionaries is often greater than their knowledge and wisdom, and has led them to destroy the existence and recollection of those relics and traditions which, in conjunction with the affinities of language, afford the best, it may almost be said the only, materials amongst which we can hope to find any satisfactory solution of the obscure question of the origin and history of the nations alluded to. They seem to flatter themselves that they are doing God service when they destroy anything which has been connected with the idolatrous worship and superstitions of the uncivilized nations, and they prompt their half-instructed converts to continue the work. They seem to forget that the worship of images is not the besetting sin of the present day as it was when the children of Israel were continually giving way to it. On the contrary, it is out of fashion, and must inevitably yield to the rapid and general diffusion of knowledge. The enemies and rivals which obstruct the progress of true religion in the present day, are to be found in the idols of our own imaginations, and in our own hearts' lusts; but some are also to be found in the skilfully directed attacks of sceptics whose fine talents and cultivated abilities are prostituted to the evil work of undermining the sacred authority of religion. The most powerful of such attacks are those which are set forth in the guise of appeals to matters of fact. The enlightened advocate of the cause of Christianity must therefore look with jealousy at the destruction of whatever may tend to throw light on those points on which he is at issue with the sceptic. Amongst these he must regard everything which tends to elucidate those parts of the history of the human race which are most obscure. He must therefore value and desire to preserve rather than seek to destroy the works of art, traditions, and mythology of uncivilized nations. He will rejoice in the industrious collection and preservation of them, and be more zealous in accumulating facts of this description than in employing his imagination in devising explanations from imperfect data, the fallacy of which time will reveal, to the injury of the cause which he had inexpertly espoused. If we can succeed in placing this subject in its proper light before the Missionary Societies, we shall have

rendered them an important service, and receive in return the most valuable cooperation which we can at present seek.

If missionaries have the means of obtaining the most minute and accurate information in consequence of their longer residence amongst the natives of uncivilized countries, the captains and medical men serving on board our merchant vessels have the advantage of affording more frequent opportunities of communication; and from the circumstance of their visiting several tribes in succession, they have facilities for instituting comparisons and discovering resemblances, provided their attention were turned to the subject, which the missionary confined to a particular spot does not possess. We should therefore do well to seek the assistance of merchants, both individually and in their associations; and from their well-known liberality and benevolence we may reasonably hope that the appeal will not be made in vain. Several mercantile expeditions have already made important contributions to our stock of knowledge of the description to which I am alluding; and were the appeal which I have just proposed at all generally attended to, materials would be more rapidly collected than by any other process which could be suggested, and at the same time a great collateral advantage would be gained in the improvement which it would be likely to produce in the treatment which the uncivilized nations too often receive from the crews of our merchant vessels, whose abuses in this respect appear to be most supinely and culpably neglected.

Another method of promoting the object which I have had in view would be to bring home to this country the living languages themselves, which would give to the masters of philological research a better opportunity of pursuing their investigations than they could find in vocabularies collected by travellers, even though more care were to be taken in obtaining them than perhaps has hitherto been generally the case. The advantages of the plan proposed, namely, that of bringing home living languages themselves in the mouths of the natives, would very much depend on the care which should be exercised in the selection of individuals: the object should be to choose the most intelligent. Where missionaries and others have succeeded in introducing schools, the best mode of selection would probably be to take two or three of the most intelligent and best-informed, who might be brought forward by a regular competition or *concours*. The voyage to Europe, being thus contended for like an open fellowship at an university, would afford a great stimulus to the scholars in these schools, and would therefore benefit the people from whom the lads were selected, whilst it

would serve the cause of Philology. In the execution of this plan, it is very probable that the liberality of the Admiralty and of merchants owning vessels might most materially assist. A combination of efforts for this purpose might form the basis of an institution something like the Propaganda at Rome, where young men from various countries are educated and trained to become influential missionaries amongst their countrymen. The great importance which the most politic leaders in the See of Rome attach to this institution is a strong proof of the influence which it is capable of exerting, and it is somewhat surprising that this example has remained so long without imitation amongst Protestant nations. It would not, however, be necessary to wait for the formation of a magnificent establishment like the Propaganda in order to act on the suggestions which I have offered. The Managers of the London University, whose liberality supplies us with the means of meeting as we do this evening, might possibly be induced to give to foreign youths, properly selected, free admission to the classes; and the missionary societies and other associations under whose auspices such youths were brought to this country, might easily provide for their suitable accommodaticn in the vicinity of the University.

If I am not greatly mistaken, the united operation of the different plans which I have proposed would speedily rescue much valuable matter from the irretrievable destruction to which it is now hastening; a stimulus would be given to a most important branch of Philology, which would give it a new position amongst the sciences, and it would acquire a popularity which it had never before possessed. It is not every one who is formed by nature to become a great Philologist: for the formation of such a character, it is essential that the individual should possess an incommunicable talent for the acquisition of languages, which seems analogous to that possessed by other individuals who almost intuitively perform the most abstruse and laborious arithmetical calculations. Examples of such linguists have been seen in Sir William Jones, the celebrated Messofanti of Bologna, William and Alexander Humboldt, Dr. Morrison, whose recent death has cast a gloom over the friends to the religious improvement of Asia, and many others; but, I cannot close the list without mentioning the name of my late friend John Fowler Hull, who when he was scarcely twenty-five years of age had made himself familiarly acquainted with nearly thirty languages, when he fell a victim to the zeal with which he pursued in India his philological researches into the ancient and modern languages of that country. The talent for the acquisition and investigation of languages differs

in one important particular at least from that which is exhibited in the performance of mathematical calculation to which I have compared it. Number in the abstract is present to every one as a material with which he may work; it is not so with language. The extraordinary genius whose talent lies in the direction of language is at the mercy of accidental circumstances for the development of his powers. Yet we may judge how strong is the passion which actuates such an individual when we see how much has been done with a stray leaf which has been torn off from some foreign work. Even those who are placed under ordinarily favourable circumstances have their attention limited to a comparatively few languages, and those with which they commence, being generally dead languages, are perhaps in the first instance more calculated to damp than to encourage their zeal. When this difficulty is surmounted, they find so vast an amount of intellectual treasure, to which the acquisition of those languages has opened the way, that they may feel little inducement to desist from their attention to these languages in favour of others of less promising appearance. Nevertheless, the greatly increased extent which has been given to the study of the Oriental languages, the renewed attention which is paid to Anglo-Saxon, and the occasional appearance of works connected even with the languages of uncivilized nations, are amply sufficient to prove the possibility of increasing the extent of philological inquiry, and of procuring a host of zealous students, who, if they should not find their labours rewarded with classical treasures to be compared with those of Greece which tell

> "...of Thebes or Pelops' line,
> Or the tale of Troy divine,"

may yet enjoy the consciousness that they open the way to a clearer and more comprehensive view of our species as a whole; that they are substituting fact for visionary speculation on a most important subject; and that in calling increased attention to the feeble, the abused, and perishing branches of the human family, they may be the means of arousing a powerful influence in their behalf, whereby they may be rescued from annihilation, and given to participate in the highest attributes and choicest privileges of our nature. Such, Gentlemen, is the prospect which, with perhaps too sanguine eyes, I have imagined that I see before us in the quarter to which I have ventured to direct your attention this evening; I have only to add, that if you think with me that there is anything feasible in the suggestions which I have offered, I would earnestly solicit your cooperation in endeavouring to carry them into execution*.

* The Philological Society has appointed a committee to draw up a set of queries in conformity with the suggestion contained in the preceding paper. When completed, they will be published in a future number of this journal.

XII. *On Magneto-electric Induction.* *By* F. Watkins.

To Richard Phillips, Esq., F.R.S. &c.

My dear Sir,

HAVING lately observed in the Philosophical Magazine some descriptions of slight modifications of apparatus for the development of magneto-electrical phænomena, and presuming from their insertion that you think such contributions interest your readers, I venture to offer to your notice an account of a philosophical apparatus, or toy, which I have contrived, and which I believe to be novel.

The production of motion by magneto-electricity is not new, many philosophers having already suggested and prepared various mechanical contrivances by which a body might be made to move continuously by magneto-electric agency.

Among the contrivances with which I am acquainted, none can vie, either in simplicity or in beauty of design, with that which emanated from the ingenuity of Mr. Saxton. The instrument as originally constructed by him may be daily seen in operation at the Gallery of Practical Science in Adelaide Street.

Having been, as you know, for a long time extensively engaged in the construction of electro-dynamic and magneto-electrical apparatus, on seeing Mr. Saxton's machine, I, with his permission, immediately commenced making one nearly after his fashion, and afterwards conceived that it might be made to show an increased number of phænomena. Following out my ideas experimentally, I obtained distinct revolutions from eight magnetic needles, together with the vibration of a ninth. I am not aware that a multiplication of motion to this extent has been achieved before; indeed, by applying a second electro-magnet seventeen bodies might be put in motion at the same time, and by a judicious arrangement even more.

The drawing which accompanies this communication represents my apparatus. In arrangement it varies very little from that of Mr. Saxton's, the difference being merely in this respect, that Mr. Saxton places the *axis* which carries his *main* revolving permanent magnet *outside* of the electro-magnet, while my axis is situated *inside*. The only advantage I obtain is that the apparatus is much more compact. Were this all I have to advance on the subject, I should not trouble you with the present communication; but as I have added seven permanent magnets in different situations, and succeeded in obtaining continued rotatory motion in all, I conceive

that I have thus rendered the magnetic toy somewhat more interesting, and am therefore induced to think it worthy of notice in your valuable Journal.

The revolving magnets I have had in motion for eleven hours without superintendence, and they were only stopped when my workshop was closed for the day. The chemical action on the copper and zinc elements of the voltaic battery employed to induce polarity in the soft iron by means of the copper wire surrounding it, is produced merely by salt and water (not nearly so strong as sea water); and I have a solution of this kind constantly in use, which has been mixed above a month, and when the metallic elements are now placed in it, the magneto-electric machine in question acts without sensible diminution of force.

The pendulum and suspended magnetic needles of this toy at times exhibit in a modified form a beautiful experiment of M. Plateau recorded in *Correspondance Mathématique et Physique*, par M. Quetelet, tom. vi. p. 70 (1830.); and as some of your readers may be curious on this matter, and not be able to obtain a sight of the work, you will probably insert the translation. I stop for a moment to state that my attention was originally called to the article in the foreign Journal through the kindness of Mr. Babbage.

M. Plateau heads his paper thus: "*On the action which a bar magnet exerts on a magnetic needle moving in a parallel plane above it.*" He then goes on to say: "Arrange a bar magnet so that it can turn in a horizontal plane about an axis passing through its centre, and place above this bar a magnetic needle sustained on a pivot, or suspended by a thread devoid of torsion. If you cause the bar to rotate slowly, the needle (as one would expect) follows and turns in the same direction with it; but if you augment the rotation of the bar to a certain point, the needle ceases to go all round, but vibrates in large arcs: on increasing still more the velocity of the rotations of the bar, the vibrations of the needle diminish in amplitude; and at length, at another certain and greater degree of velocity, it is found they cease altogether, and the needle reposes in the magnetic meridian just as quietly as if the bar were not present or in motion. This fact tends to prove that the transmission of the magnetic action is not instantaneous; and M. Plateau asks whether it could not be employed to measure the necessary time for the development of magnetic influence by induction in its full effect."

M. Quetelet, the editor, then adds a note to the following effect: "It seems to us that M. Plateau has very well pointed out the advantage we might derive from this very simple ex-

periment. We invite him to observe if the results he obtains in estimating the slowness of the transmission of the magnetic action accord with those which Messrs. Herschel and Babbage (in following another method) have found to be the case in England."

It will be observed that in M. Plateau's experiment a given velocity of rotation in the one magnet gives equal velocity of rotation to the second magnet. Increase the velocity, and the magnet acted upon, instead of revolving, vibrates, and the amplitude of the arc of vibration is greater or less as the velocity of the rotating magnet placed under it is increased or decreased. Again, we have another velocity more rapid, which with its speed does not affect the suspended needle at all, and it remains in its natural position just the same as if no moving magnetic body were in its vicinity.

The revolving magnetic needles to which I have called your attention, when they are all fairly started with the same velocity of rotation (or nearly so), act well, and continue to rotate *merrily*. But if the main revolving magnet (I mean the one which is affixed to the axis carrying the points, which by dipping into the mercury cup, hereafter to be described, cause the change in the poles of the electro-magnet more or less,) is in very rapid motion, then those magnetic needles which do not acquire the required speed at the moment of starting simply oscillate. The amplitude of the arcs of oscillation, depend I conceive, as indeed is made apparent, on the same cause with that which effected the phænomena recorded by M. Plateau. I have seen the magnetic needles frequently in perfect repose, at other times slightly tremulous, when the main revolving permanent magnet was rotating rapidly, affecting of course the rapidity of the change (or perhaps partial change) in the polarity of the electro-magnet.

The main revolving permanent magnet does not influence the revolution of the magnetic needles, as might be supposed by some persons on first inspection of the apparatus, for it may be put in rotation in an opposite direction: we may therefore justly conclude that the inductive influence of the electric current on the soft iron, is the principal cause of the continuous rotation of one and all of the rotating magnetic needles. Moreover, this apparatus, with M. Plateau's experiment, tends to prove that Messrs. Herschel and Babbage's law is just, viz. *that time is an essential element of induction.*

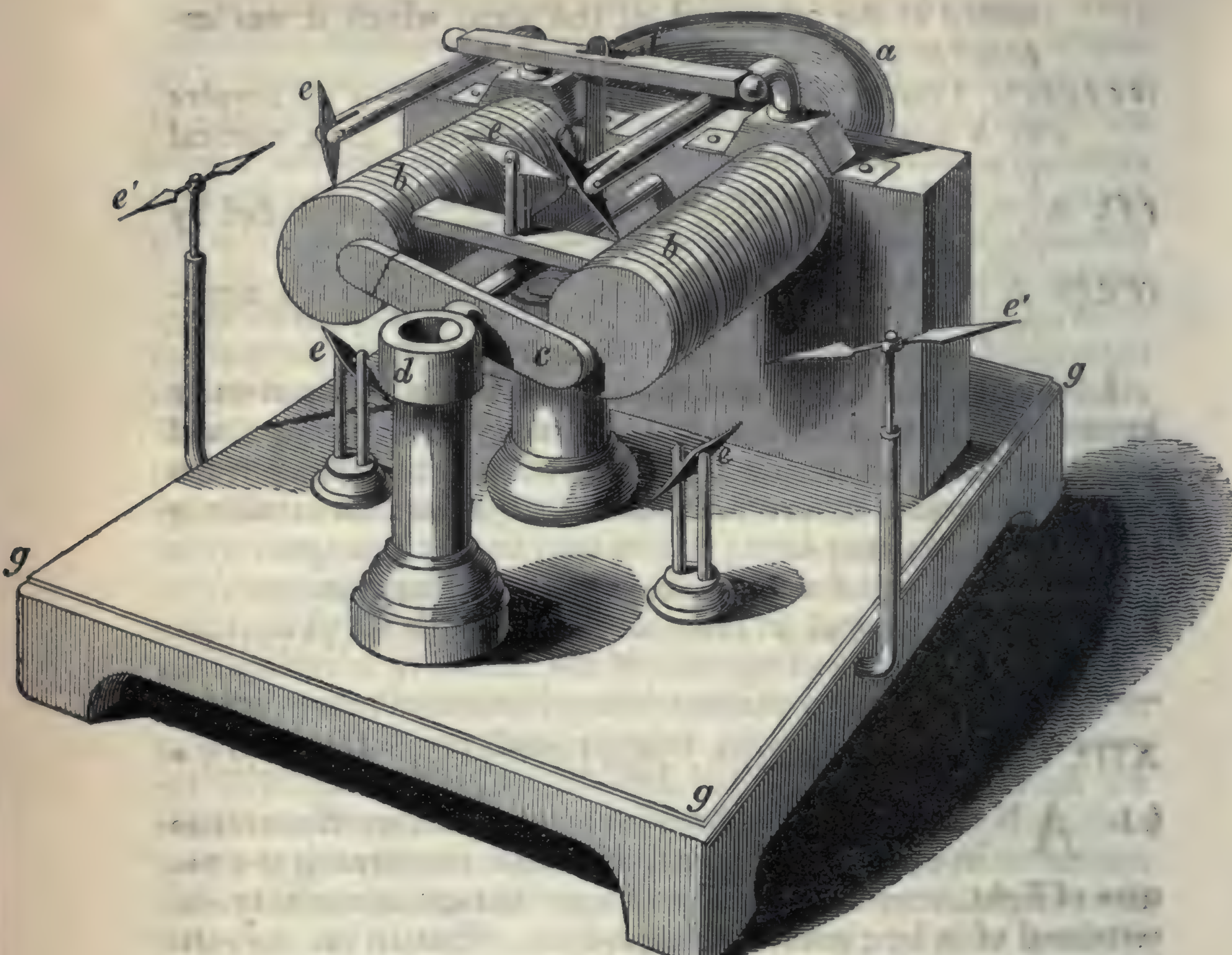
With these views it may with propriety be asked how it is that some or all of the magnetic needles, having acquired the same velocity with that of the main revolving permanent

magnet, should continue to revolve when the latter goes *very* fast? The only reply I presume to make to the question is, that I suppose, from the needles being very light and delicately suspended, when once put into rapid motion they continue for some time to revolve, by the momentum they have acquired receiving occasionally a fresh impulse by attraction or repulsion from the electro-magnet, according to the conditions in which that magnet may be at the moment. The instantaneous impulse might not be of a force equal to actuate a large needle through a space sufficiently great so as to bring the other pole of the needle within the sphere of the influence of the changed pole of the electro-magnet, or at least in proper time; therefore vibration or total rest is the consequence. Indeed, I have commonly noticed that when the points of suspension were in a defective state, even with my small and light magnetic needles they would not revolve when the poles of the electro-magnet were changed very rapidly. Another reason may be assumed for the apparent anomaly, viz. that the magnetic needles, when once revolving, keep revolving at the proper velocity by being acted upon by the *maximum* influence of one particular pole.

To make my meaning more intelligible, I shall offer, for instance, the magnetic machine now commonly used to exhibit electrical phænomena. We will take decomposition of water for an example: this has been until a recent period generally effected by voltaic electricity, in which it is supposed that a *continuous current in one direction* is constantly flowing so long as the voltaic battery is in action. The magnetic machine as originally made by Mr. Saxton consists of a revolving soft-iron armature before the poles of a permanent steel magnet, the armature being surrounded by copper wire covered with silk. Now it is clear in this arrangement that the steel magnet induces magnetism on the soft iron, that soft iron again inducing magnetism or electricity, if we like to call it so, on the copper wire which is coiled around it, and it is the recomposition or decomposition of the electrical equilibrium in this copper wire which exhibits the electrical phænomena so beautifully shown by the magnetic machine. Mr. Faraday, the parent of magneto-electrical science, has shown that when a steel permanent magnet was presented to a metal wire, its electrical equilibrium was disturbed; and provided the ends of the wire were in connexion with a measure of the disturbance, say a delicate galvanoscope, a sensible effect was produced, and a deflection of the magnetic needle of the galvanoscope took place in one direction; but this eminent

philosopher observed that the effect was instantaneous, for the needle returned to its natural position, and remained stationary notwithstanding the proximity of the magnet to the wire. But on removing the magnet from the wire, then a distinct and separate action was seen on the galvanoscope, for the needle again was deflected as far from its true position as in the first instance, but in a contrary direction. This I take to be the fundamental experiment of magneto-electricity.

Well, then, what do we do with our most improved magnetic machines? Why, instead of approaching a permanent steel magnet to a metal wire, we coil around a soft-iron armature a large quantity of wire, by that means multiplying the effects; and then, by mechanical contrivances, make the soft-iron armature revolve before the poles of a permanent steel magnet. The armature being of soft iron is only a magnet by induction when opposite the poles of the permanent magnet, and when *slowly* removed it loses all its magnetism. This is accomplished when the long axis of the armature is vertical, the axis of the permanent magnet being horizontal; therefore the armature changes its poles twice in each revolution. Hence we have in every slow revolution two actions and two reactions. One of these actions, it is true, tends to the same direction as one of the reactions; but still we have two directions of the current, and these two are antagonizing, therefore we have no TRUE polar decomposition. Yet if the soft-iron armature be made to revolve rapidly, and two vessels be employed to collect the gases from the decomposition of water, their volumes are often found to be nearly as two to one. The induced electricity cannot be said to be always in one direction. Hence, as with my constantly revolving magnetic needles, the effect must be due to the maximum of effects from one or the other pole. In other words, that one extremity of the soft-iron armature having acquired a particular polar state by induction, and time being the essential element of that state, although removed from the inducing pole and presented immediately to another pole in the opposite state, the period has not elapsed which is necessary for it to lose all its polarity before it is again brought into approximation with the first or original inducing pole, and there receiving a renewed impulse, and so on successively: the phænomena exhibited, although not strictly due to a current of electricity flowing in one direction, still give results analogous to a current of that description, and, as far as I can see, proceed from the maximum effects I have before alluded to. I deliver these observations with great diffidence, but in my mind the reasoning here given is such as is warranted by the facts before us.



Description of the Apparatus.

(*a*). A piece of soft iron bent in the form of a horse-shoe magnet, partly surrounded as at *b b* by copper wire covered with silk in the usual manner.

(*c*). A permanent magnetic needle revolving on an axis as represented in the figure, which axis has a contrivance of points dipping successively into a divided cup of mercury, one division of which is in connexion with the copper element of a voltaic circuit, and the other in connexion with the zinc element. The cup for the mercury cannot conveniently be shown in this figure, but it is placed so that the points on the axis, which have the effect of changing the current in the copper wire enveloping each arm or branch of the soft iron, may dip into it successively as the axis rotates.

(*d*). A cup of mercury connected with one end of the copper wire coiled on the arms of the soft iron, while the other end of the wire is immersed in a similar cup si-

tuated at the other end of the axis, which it was impossible to show in the figure.

(*eeeeee*). Traversing magnetic needles: two, *e′e′*, revolve in a horizontal plane, the five former in a vertical plane.

(*f*). A pendulum, consisting of a magnetic bar suspended by one end, which oscillates as already described.

(*ggg*). A mahogany stand or base for supporting the apparatus.

I have some other magneto-electrical facts not generally known, which at a future period I will put on paper, and submit them to your notice; and should you consider them worthy of attention, probably you will favour them with a place in the Magazine.

I remain, my dear Sir, yours &c.

5 Charing Cross, 10th June 1835. FRANCIS WATKINS.

XIII. *On the Nature of Light. By* H. F. TALBOT, *Esq., F.R.S.**

§ 1. AN attentive consideration of the modern discoveries in optics leads to certain views concerning the nature of light, very different from those that are commonly entertained of it by persons who have not reflected on the subject.

If we admit the truth of the undulatory hypothesis, it follows as a necessary consequence that light (or rather the ætherial medium whose vibrations constitute light,) is present everywhere, and at all times, ready to transmit in any direction, and to any distance, whatever vibration may be excited among its molecules. It is therefore as truly and actually present with us during the darkest night, as it is in the daytime, or even in the sunshine. This is a difficulty, and a kind of paradox, from which the doctrine of emission is free.

As, however, many other things have been found to be true which are exceedingly contrary to the first impressions of our senses, (for instance, the earth's motion,) there is no reason for rejecting this hypothesis *à priori*, if it explains other facts in a satisfactory manner.

It is remarkable that intense light is sometimes produced under circumstances where there does not appear to exist any adequate cause for its production. Every one knows the effect of placing a bit of lime in the oxyhydrogen flame; but, I believe, no satisfactory explanation has been given of it. It has been attributed to the formation and subsequent combus-

* Communicated by the Author.

tion of the metal calcium, an idea which in the first place seems contradictory in itself, and at any rate is destitute of proof. The great heat gradually dissipates the lime, and this circumstance is liable to be mistaken for a necessary consequence of the experiment instead of being an accidental accompaniment of it, which ought to be avoided if we wish to obtain an insight into the cause of the phænomenon. In order to do so, we must try the experiment at a lower temperature, at which lime is perfectly fixed. The first experiments of this kind that I know of were published by Mr. Cameron of Glasgow, and Sir David Brewster in 1820 (*vide* Edinb. Phil. Journ., vol. iii. p. 343).

The best method of proceeding is the following:

Dip a piece of white paper in a solution of muriate of lime, and then wipe off the superfluous liquid, leaving the paper a little moist. Cut a strip of it, and hold the extremity in the flame of a spirit-lamp. After a minute or two the carbonaceous matter of the paper will be dissipated, and the lime will alone remain, in the form of a perfectly white minutely divided network. This will soon become very vividly ignited and emit a bluish-white light. Practice is requisite in this experiment to obtain the most successful result. When the incandescence has become brilliant, it remains so for any length of time unaltered and without diminution as long as the lamp is supplied with alcohol.

Now, the chief thing that merits observation is, that the original weight of the lime is only a small fraction of a grain, and that no diminution of it is perceived at the end of the experiment. It is surely unnecessary to advance any further argument to prove that the light is not really emitted by the lime in the manner in which a candle emits light, by the combustion of its particles, but that it acts in some wholly different manner. In short, we see that the *mere presence* of the lime, in a heated state, is the cause of the light.

In order to know to what degree the luminosity of a flame might be increased by the presence of lime, I made the following experiment:—A flame of alcohol was gradually diminished as much as possible, by reducing the wick to a single thread, and by other contrivances.

In this state, when the minimum of combustion was attained, the flame was reduced nearly to the size of a pea, and it gave a very faint blue light. When placed in a dark room, the flame itself was visible, but nothing could be seen by its light, not even any part of the lamp itself. A particle of lime (reserved from a former experiment) was then placed in the flame. The lime immediately became incandescent, and light enough was emitted by it to tell the hour on a watch at the distance

of several yards. In this experiment I have no means of judging in what proportion the light was augmented, but considering the facts above stated, it could not have been less than several hundred times*.

I do not know whether the explanation which I am about to offer of this phænomenon has ever been suggested, at any rate I do not remember to have met with it, and therefore I will mention it here briefly. All vibrating bodies communicate their own motion to the elastic medium in which they vibrate. This is a purely mechanical effect. We see an example of it in the atmosphere, which is set in motion by any vibrating body. It is by no means necessary that there should be any affinity between the air and the sonorous body, but only that the latter should move with a certain degree of rapidity and regularity. Now, suppose the velocity of vibration increased to an immense degree: and there is no difficulty in conceiving that the moving body may then become capable of communicating its vibrations to the surrounding *æther*: in which case it will cause the formation of *waves of light*, or, in other words, it will become a luminous object. Indeed, it is difficult to deny the *theoretical possibility* of such an occurrence, without at the same time refusing to the æther the ordinary qualities of an elastic medium, and thereby abandoning all analogy between light and sound. But whether such a production of light be *practically* possible depends on whether the particles of ordinary matter are capable of executing vibrations with a rapidity at all comparable to those of light. Mechanical means are evidently too rude for the production of such an effect; but *heat*, considered as a disturbing cause of molecular equilibrium, seems to be an agent of adequate power and energy*.

I am of opinion, therefore, that the emission of intense light by a particle of lime in this experiment, without the loss of any portion of its own substance, arises from the cause above referred to, namely, that the heat throws the molecules of lime into a state of such rapid vibration that they become capable of influencing the surrounding ætherial medium, and producing in it the undulations of light.

§ 2. According to the above explanation, the heat of the alcohol flame is only necessary in order to cause vibrations of the molecules of the lime. If, therefore, any substance could be found that would vibrate when cold, it might be capable of emitting light spontaneously. This appears to me a plausible explanation of what is commonly termed phosphorescence. Solar phosphori are those which, having been ex-

[* See our "*Scientific Intelligence and Miscellaneous Articles*" in the present Number.—Edit.]

posed to the sun's rays, continue to give light when removed into a dark apartment. Some important observations concerning them have been lately published by Osann*. He found that no augmentation of their light took place in oxygen gas, nor any diminution of it in hydrogen gas; so that no combustion existed, nor was there any sign of it. But is there any difficulty in supposing that the molecules of a substance, after having been caused to vibrate powerfully by the sun's rays, may afterwards continue to move spontaneously for a certain time when left to themselves? If any doubt should exist as to the *possibility* of the thing, I think it will be removed by the experiment which I am about to mention. A sheet of paper was moistened with a solution of nitrate of silver, a substance which, it is well known, is capable of being blackened by the influence of solar light. Half of the paper was covered, and half exposed to sunshine; but owing to its being a dull day in the winter season, no effect was produced. After several minutes the paper was removed, and being examined, showed hardly any perceptible difference between the part that had been covered and that which had been exposed to the sun. It was then removed to another room, where the sun does not shine in the winter season, and accidentally left on a table exposed to the daylight. Some hours afterwards I was surprised to find that the paper had become partially darkened, and that the dark part was that which had been previously but ineffectually exposed to the sunshine, while the other part still retained much of its original whiteness. This anomalous fact, of which I could find no explanation at the time, appears to me now to be closely connected with what I have advanced as a probable cause of phosphorescence.

In the first place, white paper is known to be a weak solar phosphorus, and even were it not so, we have here an indubitable proof, that in consequence of its exposure to the solar rays a spontaneous action *of some sort or other* commenced, and continued for some hours. Putting the facts together, I see no great improbability in supposing that the spontaneous action which is sufficient in one case partially to decompose nitrate of silver, may be sufficient in another case to act upon light so as to produce what we call phosphorescence. If this idea is admitted, the cause of the ultimate cessation of phosphorescence may be, that the vibrating particles at length arrange themselves in positions of stability; from which, however, they may be again deranged by a new impulse, as, for instance, by an electric shock. Mr. Pearsall has, in fact, discovered that extinct phosphorescence is powerfully revived by electric discharges. Moreover, it has been found by experi-

* *Poggendorf's Annals*, New Series, vol. iii. p. 405.

ment that only the violet rays are effective in producing phosphorescence. This is confirmed by the recent experiments of Osann. The violet rays also cause the discolouration of the nitrate of silver, the rest of the spectrum being nearly ineffective. This analogy is much in favour of my argument.

§ 3. I come now to consider what probable conclusions can be drawn respecting the nature of light from the numerous absorptions which it exhibits in passing through nitrous gas, and the other coloured gases. For the discovery of this most remarkable phænomenon it is well known that science is indebted to Sir David Brewster. The absorptions or dark lines in the spectrum produced by iodine vapour, are described by Professor Miller *, as being about a hundred in number, and equidistant. I have, however, found by careful observations that they are not equidistant, but that they become gradually more crowded towards the blue end of the spectrum. Baron von Wrede † has observed the same fact, and established it by actual measurement.

This approximation of the lines takes place gradually, and seems a consequence of some simple general law. There is also a space at each extremity of the spectrum in which no lines are visible. Now, in the former part of this paper I have advanced the hypothesis that the vibrations of light and those of material molecules are capable of mutually influencing each other. It remains to be seen, whether the same hypothesis does not afford a clue to the explanation of this apparently complex phænomenon of absorption.

It is known that certain gases combine rapidly when exposed to sunshine, which do not unite in the dark: no doubt because the light causes the molecules to vibrate, so as to come within the sphere of each other's attraction.

I conclude from this, and many other facts of the same kind, that light when traversing a transparent medium is able to excite motion among its particles. This being admitted, let us suppose iodine vapour so constituted that its molecules are disposed to vibrate with a rapidity not altogether dissimilar to that of light. Now, if the differently coloured rays differ also (as is probable) in rapidity of vibration, some of them will vibrate *in accordance*, and others *in discordance*, with the vibrations of the iodine gas. And these accordances and discordances will succeed each other in regular order, from the red end of the spectrum to the violet end; each discordance being marked by a dark line or deficiency in the spectrum, because the corresponding ray is not able to vibrate through the medium, but is arrested by it and absorbed. I communi-

* Lond. and Edinb. Phil. Mag., vol. ii. p. 381.

† *Poggendorf's Annals*, vol. iii. p. 353.

cated this hypothesis two or three years ago to Sir John Herschel, as affording at least a partial explanation of the phænomenon, and moreover deducing it from a very simple cause.

Baron von Wrede has lately published some speculations upon the same subject, which at first sight seemed to me coincident with my own. But his hypothesis is different from mine. He supposes the particles of iodine vapour to be motionless, and that they act upon the light as minute reflective planes situated at small and equal distances from each other. According, therefore, to the usual laws of the interference of luminous waves, some are transmitted and others destroyed, according to the length of their undulations compared with the distance between the particles of vapour.

XIV. *Suggestions respecting the ensuing Meeting of the British Association for the Advancement of Science.* By A Correspondent.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE meeting of the British Association which was held last year in Edinburgh, will long be remembered with pleasure, for the warmth with which its objects were patronized, and for the friendly feelings and liberal hospitality so uniformly evinced to the members.

We are next month to visit our open-minded and kind-hearted neighbours the Irish; and I look forward with the most lively satisfaction to the occasion. Our institution has made some splendid acquisitions in the sister isle; for some of the most worthy and talented of her sons have been among the first and most anxious to give it their sanction. We owe much to the character and abilities of these gentlemen; but still more to their zeal and energy, which have so nobly seconded the exertions of the founders of the British Association, and have thus given an impulse to the society, of which every one is happy in feeling and in acknowledging the influence.

I trust, therefore, that the assemblage in Dublin will be ample; and that there will be a general disposition manifested among the more influential of our members to make every effort to attend it*. The invitation has been cordial, and its

* We add a note to inform our readers of the kind and public-spirited resolution of Sir John Tobin to devote the finest steam-vessel in the port of Liverpool to the accommodation of such Fellows of the Royal Society, being also members of the Association, to whom it may be convenient to proceed from thence to Dublin on the 9th of August. The communication by which we are authorized to make known this most liberal invitation has just reached us, and will be found among the Correspondence, on the cover of the present Number.—Edit.

acceptance will be gratifying. "Est enim—valde decorum patere domos hominum illustrium, illustris hospitibus; idque, etiam reipublicæ est ornamento, homines externos, hoc liberalitatis genere,—non egere."

The British Association is a valuable appendage to the institutions of the country; and I hope I may be excused, as one very zealous for its welfare, in submitting to the consideration of its principal managers a few suggestions relative to some of the arrangements connected with its meetings.

The scientific proceedings of the society must necessarily be very much conducted in the sections; but it appears to me that the accounts which are given at the general meetings, of the proceedings of the sections, instead of being almost entirely confined, as has generally been the case, to a statement of the *subjects* only of communication, should contain an analysis of the more interesting of those communications, given in a concise and intelligible form. I need hardly refer, as an example of the mode of framing such analyses, to the Royal and other societies, where a short and perspicuous *exposé* is uniformly given of such papers as may have been read at the previous meetings.

The abstract which is so properly invited by the Association to accompany any investigations of considerable length, would assist materially in the formation of the analysis to which I have alluded; and though there would be labour and difficulty attendant on the plan proposed, yet I would submit that whatever is laid before the public meetings, should be brought forward in a form which may be as likely as possible to be satisfactory to the great bulk of the members; and should always have the advantage of being communicated with adequate powers of voice, and distinctness of enunciation.

If the plan be persevered in, which was pursued at the Edinburgh meeting, of confining scientific communications entirely to the sections, it is the more necessary that some such measures as those which I have pointed out should be called into action, in order to give sufficient interest to our public meetings; but I would venture to suggest, whether it would not be expedient and advantageous, at such meetings, to have some communications read, of moderate length, comprehensive views, and at the same time popular structure, with which to animate and instruct the assembly. It would not be an unworthy condescension in the most highly gifted of our members to lend themselves occasionally to such an object; though I am aware that some may be disposed to regard it as a little beneath the dignity of science to mix up anything of the *popular* with our proceedings. It is, how-

ever, to the mode of exposition that I would apply the term,—to science popularly developed,—not to what may be called popular science; and I need not point out to men of accurate knowledge and of clear and comprehensive views, that some of the most important and refined objects of inquiry may be treated in apt, plain, and intelligible language, and elucidated by lively and appropriate illustrations.

Our Society is not composed of philosophers alone. It enrols among its members, as has been well stated of the Yorkshire Philosophical Society, "not only those who are themselves engaged in philosophical studies, but all by whom the value of such studies is duly appreciated; not those alone who hope to extend the boundaries of knowledge by their own researches, but all who are willing to encourage the prosecution of such researches by others, and to concur in furnishing those facilities to scientific inquiries which a philosophical association is capable of affording."

We therefore invite additions to our members, from the respectable classes of society, wherever we go, without being nicely inquisitive as to qualifications; and we are right in doing so, for, independently of the pecuniary resources which they afford, there is an animating and exhilarating influence in numbers, of which it is impossible that the gravest and most philosophical can be insensible. But with such views of the elements of the Society, it is important to bear in mind, in its proceedings, the popular as well as the philosophical character of its structure.

The British Association derives its origin from the celebrated meeting of scientific men which annually takes place in different cities of Germany; but it was thought desirable, at its formation, to aim at making it more subservient to the advancement of science than that on the Continent professes to be. The views of its founders were so honourable and high-minded as to merit every possible support and encouragement from the scientific world; but in thus endeavouring to place the objects of the Society in a higher and more dignified position than those of its parent, I trust it will always be borne in mind, how essential the *attendance* of its principal members is to the preservation of the main characteristic of the institution, that of bringing kindred spirits together, and thus connecting the pursuits of literature and science with the kindliest feelings of our nature: "Omnium Societatum nulla præstantior est, nulla firmior, quàm cùm viri boni, moribus similes, sunt familiaritate conjuncti;" and I venture to submit, that the most successful production which the most able of our members may furnish, in obedience to the wishes of the Society, is not equal in its moral influence, as a

means of fostering and extending the love of literature and science, and promoting its permanent advancement, to the easy colloquial intercourse to which our annual convocations give rise. The most elaborate and valuable reports are but little known, and but imperfectly appreciated during the meetings of the Association. They are for perusal in the closet; and the full advantage of them is to be obtained whether the reader attend our meetings or not. If the most material portion of the operations of the Society is, therefore, to be viewed as connected with the production of reports, or even the following up of important and interesting subjects of investigation, there is but little inducement by this means furnished to personal attendance; and it is in the judicious encouragement to such attendance that, as it appears to me, the life's blood of the Society consists. The Continental meetings have been eminently successful; and I should be sorry that the plan of them, which has stood the test of many years' experience, should be too much departed from; for I should fear that in considering the expediency of a distant expedition, the scale would hardly descend in its favour if the prime object of our meetings were merely to afford certain specified occupations for the intervals between them.

The annual meeting of the British Association may with propriety be regarded as a kind of Saturnalia in science, as a period of relaxation and enjoyment, no less favourable to the preservation and augmentation of intellectual vigour, than well-regulated exercise, and change of scene and avocation, are to bodily health. Too much stress, I would venture to submit, has been laid on the *direct*, and too little on the *indirect*, influence of the Society, in the advancement of science; for the unreserved and easy association of philosophical minds has not only a tendency to remove prejudices and correct errors, but to strike out new trains of thought, to communicate new facts, and to suggest improved modes of inquiry in every department of human knowledge. The result of a single meeting may thus, in fact, prepare for years of investigation and research; and it is this indirect influence on the interests of science which constitutes the particular charm of the Society, and its peculiar distinction from other scientific associations. I need say nothing of the tendency of our institution to keep up or revive old friendships, and to generate new ones; to produce an acquaintance with the most agreeable parts of the human character; and to bring together men differing, perhaps, widely in some of the most important points of opinion, on a sort of neutral ground,

where all is amity and goodwill. Who, likewise, does not wish to be acquainted with the philosopher in common life? to know something of his character and manners, and how he comports himself in the varied and interesting relations of society? In former times there seemed to be something of repulsion in the deeper investigations of science, which kept their cultivators on a sort of hallowed ground. In the retirement and seclusion of the college or the cloisters, the man of literature and science possessed a kind of superior and separate existence. But as we know him better, awe and reverence give place to love and esteem; and we are gladdened to find that those who can enlighten by their erudition, can charm by their wit; and that the same mind which is distinguished for the grave severity of its philosophical pursuits, can unbend itself to delight by vivacity, good temper, and willing participation in the feelings and occupations of common life.

No man is a hero to his *valet de chambre*, and no man is a philosopher to his *gyp*; but if close approximation is unfavourable to the *ignotum-pro-magnifico* feeling; if distance gives mellowness of prospect, and that sort of enchantment on which poets love to dwell; minuteness of observation, on the other hand, exhibits nicety of structure and beauty of appearance which art in vain attempts to imitate, and often discovers amid the infinite peculiarities of human character, virtues and excellences which elude cursory and general observation.

I submit these observations with every feeling of respect and deference, and with the most earnest wish that our goodly structure may go on and prosper. We have nearly arrived at an important epoch in our existence, that in which, having gone through the universities of the United Kingdom, we are to visit some of its principal cities; and if anything more can be done, than has already been attempted, to give greater effect to the operations of the Society, particularly by encouraging the afflux of foreigners as well as our countrymen at its meetings, I have every confidence that the principal members of the Council, to whom the Society has already been so deeply indebted, will give every attention to the subject.

At the formation of the British Association, I have understood that one of the most distinguished members of our Society suggested, that the plan of the Continental meetings should be followed in establishing it. I know not what Sir David Brewster's opinion may still be upon this point, but at all events it is right for me to state that I have not the honour of his

acquaintance, and that my letter is written without his knowledge, except, perhaps, as one of the Editors of your valuable work. I have the honour to be,

Gentlemen, yours, &c.,

July 18, 1835. X. Y. Z.

XV. *On* Olbers's *Method of determining the Orbits of Comets. By Professor* ENCKE.

[Continued from p. 25.]

THE general differential equations for every celestial body of our system, if x' and y' denote its heliocentric coordinates in the plane of its orbit, are

$$\frac{d^2 x'}{d t^2} + \frac{x'}{r'^3} = 0 \qquad \frac{d^2 y'}{d t^2} + \frac{y'}{r'^3} = 0$$

in which the terms are supposed already to have been multiplied by the constant K. These equations give the following by further differentiation:

$$\frac{d^3 x'}{d t^3} = + \frac{3 \frac{d r'}{d t}}{r'^4} x' - \frac{1}{r'^3} \cdot \frac{d x'}{d t}$$

$$\frac{d^4 x'}{d t^4} = + \left\{ \frac{1}{r'^6} - \frac{12}{r'^5} \left(\frac{d r'}{d t} \right)^2 + \frac{3}{r'^4} \cdot \frac{d^2 r'}{d t^2} \right\} x'$$

$$+ \frac{6}{r'^4} \cdot \frac{d r'}{d t} \cdot \frac{d x'}{d t}$$

and similar expressions for $\frac{d^3 y'}{d t^3}$ and $\frac{d^4 y'}{d t^4}$ by changing x' for y'. If these values are substituted in Taylor's formula, every x and y may be expressed by a series in which only the first differentials of the coordinates are to be found, and in which the coefficients of x' and $\frac{d x'}{d t}$ (which coefficients involve r' and its differentials together with the powers of the time) in the expression for x are identical with the coefficients of y' and $\frac{d y'}{d t}$ in the expression for y.

Putting, for brevity,

$$\text{(25)} \quad \ldots\ldots\ldots \quad \begin{aligned} K(t' - t) &= \tau'' \\ K(t'' - t') &= \tau \\ K(t'' - t) &= \tau' \end{aligned}$$

and likewise,

$$w_{,} = 1 - \tfrac{1}{2}\frac{\tau''^2}{r'^3} - \tfrac{1}{2}\frac{\tau''^3}{r'^4}\frac{d\,r'}{d\,t} + \tfrac{1}{24}\left\{\frac{1}{r'^6} - \frac{12}{r'^5}\left(\frac{d\,r'}{d\,t}\right)^2 + \frac{3}{r'^4}\frac{d^2\,r'}{d\,t^2}\right\}\tau''^4$$

$$w_{,,} = \tau'' - \tfrac{1}{6}\frac{\tau''^3}{r'^3} - \tfrac{1}{4}\frac{\tau''^4}{r'^4}\frac{d\,r'}{d\,t}$$

$$w' = 1 - \tfrac{1}{2}\frac{\tau^2}{r'^3} + \tfrac{1}{2}\frac{\tau^3}{r'^4}\frac{d\,r'}{d\,t} + \tfrac{1}{24}\left\{\frac{1}{r'^6} - \frac{12}{r'^5}\left(\frac{d\,r'}{d\,t}\right)^2 + \frac{3}{r'^4}\frac{d^2\,r'}{d\,t^2}\right\}\tau^4$$

$$w'' = \tau - \tfrac{1}{6}\frac{\tau^3}{r'^3} + \tfrac{1}{4}\frac{\tau^4}{r'^4}\frac{d\,r'}{d\,t},$$

the expressions for the cordinates of the first and third places become

$$x = w_{,}\,x' - w_{,,}\frac{d\,x'}{d\,t}$$

$$y = w_{,}\,y' - w_{,,}\frac{d\,y}{d\,t}$$

$$x'' = w'\,x' + w''\frac{d\,x'}{d\,t}$$

$$y'' = w'\,y' + w''\frac{d\,y'}{d\,t}$$

accurate to the fourth power of the times inclusively. Substituting these values in the above symbolic expressions $[r\,r']$, $[r\,r'']$, $[r'\,r'']$, for the quantities $y'\,x - x'\,y$, $y''\,x - x''\,y$, $y''\,x' - x''\,y'$, and remembering that $x'\,\frac{d\,y'}{d\,t} - y'\,\frac{d\,x'}{d\,t}$ is the double areal velocity, consequently, according to Kepler's law $= \sqrt{p}$, p being the semi-parameter of the orbit, we obtain

$$[r\,r'] = \sqrt{p}\left\{\tau'' - \tfrac{1}{6}\frac{\tau''^3}{r'^3} - \tfrac{1}{4}\frac{\tau''^4}{r'^4}\cdot\frac{d\,r'}{d\,t}\,\ldots\ldots\right\}$$

$$[r\,r''] = \sqrt{p}\left\{\tau' - \tfrac{1}{6}\frac{\tau'^3}{r'^3} + \tfrac{1}{4}\frac{\tau'^3(\tau - \tau'')}{r'^4}\cdot\frac{d\,r'}{d\,t}\,\ldots\right\}$$

$$[r'\,r'] = \sqrt{p}\left\{\tau - \tfrac{1}{6}\frac{\tau^3}{r'^3} + \tfrac{1}{4}\frac{\tau^4}{r'^4}\cdot\frac{d\,r'}{d\,t}\,\ldots\ldots\right\}$$

or as only the ratios of every two areas are required

$$\frac{[r'\,r'']}{[r\,r']} = \frac{\tau}{\tau''}\left\{1 - \tfrac{1}{6}\frac{\tau^2 - \tau''^2}{r'^3} + \tfrac{1}{4}\frac{\tau^3 + \tau''^3}{r'^4}\cdot\frac{d\,r'}{d\,t}\,\ldots\right\}$$

$$\frac{[r\,r'']}{[r\,r']} = \frac{\tau'}{\tau'}\left\{1 - \tfrac{1}{6}\frac{\tau'^2 - \tau''^2}{r'^3} + \tfrac{1}{4}\frac{\tau\,(\tau\,\tau' - \tau''^2)}{r'^4}\;\frac{d\,r'}{d\,t}\,\ldots\right\}$$

$$\frac{[r\,r'']}{[r'\,r'']} = \frac{\tau'}{\tau}\left\{1 - \tfrac{1}{6}\frac{\tau'^2 - \tau^2}{r'^3} + \tfrac{1}{4}\frac{\tau''(\tau^2 - \tau'\,\tau'')}{r'^4}\cdot\frac{d\,r'}{d\,t}\,\ldots\right\}$$

which are accurate to the third power of the times inclusively. These values are true for every body of our system. In the particular case of the orbit of the earth, the term of the third order becomes quite insensible, on account of the very small factor $\frac{d\,R'}{d\,t}$, and belongs, in some measure, to a higher order. The term of the second order in the only quotient which is employed in Olbers's method, viz. $\frac{[R'\,R'']}{[R\,R']}$, becomes for $\tau = \tau''$ or for equal intervals quite equal zero. As one may always approximate to this most favourable case of the intervals of the observations, as nearly as the observations from which the selection is made will permit, the most considerable part, at least, of the influence of terms of the second order may be destroyed. Putting $\tau = l\,.\,\tau''$ we have

$$\frac{[R'\,R'']}{[R\,R']} = \frac{\tau}{\tau''}\left\{1 - \tfrac{1}{6}\frac{l-1}{l+1}\cdot\frac{\tau^2}{R'^3}\cdots\right\}$$

where the factor $\frac{l-1}{l+1}$ has for $l = \frac{2}{3}$ the value of $\frac{1}{5}$, for $l = 2$ the value $\frac{1}{3}$, and for $l = 3$ the value $\frac{1}{2}$; in the case, therefore, of the very unfavourable distribution of the time in the ratio of 1 to 3, one half of the influence of the terms of the second order is annihilated.

For the comet we may find an exact expression of the ratio of the sector to the triangle by means of the auxiliary quantities which have been used for the solution of Lambert's equation, and these expressions will prove that here likewise in all cases that can occur in practice, the terms of the second order are so great in comparison of those of higher orders, that a solution satisfying the former is sufficiently accurate. The equation (6) may, agreeably to its import, be written thus:

$$F = \tau''\sqrt{2\,q} = [r\,r'] + \tfrac{1}{3}\,[r\,r']\,\frac{\sin f^2\sqrt{r\,r'}}{q\cos f}$$

or substituting from (4) and (5)

$$F = \tau''\sqrt{2\,q} = [r\,r']\left\{1 + \tfrac{1}{3}\,\frac{(m-n)^2}{m\,n}\right\}.$$

But as $$k = (r + r')\sin\gamma,$$

we shall have, by substituting this value in (3),

$$m = (\cos\tfrac{1}{2}\gamma + \sin\tfrac{1}{2}\gamma)\sqrt{(r + r')}$$
$$n = (\cos\tfrac{1}{2}\gamma - \sin\tfrac{1}{2}\gamma)\sqrt{(r + r')},$$

consequently:

$$m - n = 2\sin\tfrac{1}{2}\gamma\sqrt{(r + r')}$$
$$m\,n = \cos\gamma\,(r + r'),$$

by which we get

$$F = \tau'' \sqrt{2\,q} = [r\,r'] \left\{1 + \tfrac{4}{3}\,\frac{\sin \frac{1}{2}\,\gamma^2}{\cos \gamma}\right\}$$

$$= [r\,r'] \left\{\frac{1 + 2 \sec \gamma}{3}\right\}.$$

But as $\sin \gamma = \eta \,.\, \mu$, the factor $\frac{1 + 2 \sec \gamma}{3}$, or the ratio $\frac{F}{[r\,r']}$ may be united with the table for the solution of Lambert's equation. In the column of that table headed

$$\log \nu = \log \frac{1 + 2 \sec \gamma}{3}$$

its value for $\eta = 0$ to $\eta = 0{\cdot}32$ has been given. To continue it further appeared quite unnecessary, in consideration of the possible use which might be made of it. For the smallest value which in general may be admitted, viz. for $r + r' = 1$, $\eta = 0{\cdot}32$ corresponds already to an interval of 9 or 10 days.

Taking into account only the first five decimals log ν increases very nearly as the square of η, or for the same $r + r'$, nearly as the square of the time. If, on the contrary, we assume a certain interval which for every single sector will rarely be greater than seven days, we obtain for different $(r + r')$ the following table:

$\tau'' = 7$ days.			
$r + r'$	$\log \nu$	$r + r'$	$\log \nu$
1·0	0·00874	2·0	0·00105
1·1	650	2·1	91
1·2	497	2·2	79
1·3	389	2·3	69
1·4	310	2·4	61
1·5	252	2·5	54
1·6	207	2·6	48
1·7	172	2·7	43
1·8	145	2·8	38
1·9	123	2·9	34
2·0	105	3·0	31

Consequently log ν likewise increases very nearly in the inverse ratio of the cube of the radius vector. The terms of the second order prevail, therefore, on the whole very much. For these terms the circumstance above noticed for the orbit of the earth, viz. that for equal intervals they are entirely anni-

hilated in Olbers's method, likewise takes place, and consequently the problem is nearly rigorously solved; for intervals not quite equal the greater part of their amount at least is destroyed. These series show, likewise, the reason of Dr. Olbers's remark at the end of the preceding paper, that it is generally somewhat more accurate to take

$$\frac{[r' r'']}{[r r']} = \frac{[R' R'']}{[R R']}$$

instead of the more simple ratio of the intervals of time. The error is less as long as $r < \sqrt[3]{2}$, but when $r > \sqrt[3]{2}$ the ratio of time is to be preferred.

From the explanation here given the great success which has attended Olbers's method on almost every occasion may be sufficiently accounted for. The quantities of the first order are in every case completely taken into account. In the first place the possibility of the case of equal intervals, not a very rare one, in which even the terms of the second order with respect to the intervals are perfectly destroyed, and a rigorous solution is obtained; and in the next place the certainty that even in the cases of unequal distribution of the time between the observations only a very small part of these latter terms is neglected, insure to this method the preeminence over all the other known methods, which, as it is founded in theory, has from its very first publication been confirmed by practice.

Should it be apprehended that in cases of very unequal intervals the error of the approximate supposition might too much affect the result, this supposition may be amended as soon as the calculation has been carried on to the end of the trials. The values of r, r'' which are known, are generally correct to quantities of the second order. Deducing from them the value of r' or making

$$r' = \tfrac{1}{2}(r'' + r) - \tfrac{1}{2}\frac{\tau - \tau''}{\tau'}(r'' - r)$$

and likewise

$$\frac{d r'}{d t} = \frac{r'' - r}{\tau'}$$

we shall obtain correctly to quantities of the third order,

$$(26)\quad \frac{[r' r'']}{[r r']} = \frac{\tau}{\tau''}\left\{1 - \tfrac{4}{3}\frac{(\tau - \tau'')\tau'}{(r'' + r)^3} + 4\,\tau\tau''\,\frac{r'' - r}{(r'' + r)^4}\right\}.$$

If we calculate besides accurately from the existing data $\frac{[R' R'']}{[R R']}$ and designate the approximate value of ρ brought out

by the trials by (ρ), the value of M which is now to be considered as perfectly accurate will be

$$(27)\quad M = \frac{[r' r'']}{[r r']} M' + \left(\frac{[r' r'']}{[r r']} - \frac{[R' R'']}{[R R']}\right) \frac{R}{(\varrho)} . M''.$$

A final solution of the problem, to calculate r' from the values of r, r'' and k which have been obtained, these values involving already the determination of the whole orbit, and consequently the value of r', has been given by Bessel in Schumacher's Astronomical Memoirs (*Abhandlungen*); it leads to the solution of two cubical equations. If deemed convenient, one may likewise make use of the value of log ν given in the table, which is to be deducted from the logarithm of the interval in order to obtain that of the area of the triangle. The value of r' is thus found by the same interpolation as above.

There is, however, a case to which the method as hitherto deduced cannot be applied. A perfect understanding of it is most easily obtained by reverting to the original formulæ (14), and by comprising the values of M' and M'' in a different manner. If we conceive the direction of the sun's longitude for the second observation to be S', and the directions given by the three places of the comet to be C, C', C'' and all marked on the sphere of the heavens, and then suppose the triangles between the pole of the ecliptic and S' C, S' C', S' C'' drawn; and if we designate in each of them the sides S' C, S' C', S' C'' by σ, σ', σ'', and the angles at S' by Σ, Σ', Σ'', we have by spherical trigonometry the following six equations:

$$\begin{aligned}
\sin\sigma \sin\Sigma &= \cos\delta \sin(\alpha - \Theta') \\
\sin\sigma \cos\Sigma &= \sin\delta \\
\sin\sigma' \sin\Sigma' &= \cos\delta' \sin(\alpha' - \Theta') \\
\sin\sigma' \cos\Sigma' &= \sin\delta' \\
\sin\sigma'' \sin\Sigma'' &= \cos\delta'' \sin(\alpha'' - \Theta') \\
\sin\sigma'' \cos\Sigma'' &= \sin\delta''.
\end{aligned}$$

Deducting the product of the second and third equations from that of the first and fourth, and performing the analogous operation with the last four of them, we have

$$\sin\sigma \sin\sigma' \sin(\Sigma - \Sigma') = \cos\delta \cos\delta' \{\tan\delta' \sin(\alpha - \Theta') - \tan\delta \sin(\alpha' - \Theta')\}$$

$$\sin\sigma' \sin\sigma'' \sin(\Sigma' - \Sigma'') = \cos\delta' \cos\delta'' \{\tan\delta'' \sin(\alpha' - \Theta') - \tan\delta' \sin(\alpha'' - \Theta')\}$$

or by substituting these values in M' and M'',

$$M' = \frac{\sin\sigma \sin(\Sigma - \Sigma')}{\sin\sigma'' \sin(\Sigma' - \Sigma'')} \cdot \frac{\sec\delta}{\sec\delta''}$$

$$M'' = \frac{\cos\Sigma' \sin(\Theta' - \Theta)}{\sin\sigma'' \sin(\Sigma' - \Sigma'')} \cdot \frac{1}{\sec\delta''}.$$

The quantities σ and σ'' are independent of the intervals; but $\Sigma - \Sigma'$ and $\Sigma' - \Sigma''$ and consequently the numerators and denominators of M$'$ and M$''$ will generally be of the first order of the intervals, and their values will not be liable to greater errors than the observed $\alpha' - \alpha$ and $\delta' - \delta$. For we have

$$\sin\sigma \sin\sigma' \sin(\Sigma - \Sigma')$$
$$= \sin(\delta' - \delta)\sin\left(\tfrac{1}{2}(\alpha + \alpha') - \Theta'\right)\cos\tfrac{1}{2}(\alpha' - \alpha)$$
$$- \sin\tfrac{1}{2}(\alpha' - \alpha)\cos\left(\tfrac{1}{2}(\alpha + \alpha') - \Theta'\right)\sin(\delta' + \delta).$$

If, however, in a particular case, $\Sigma - \Sigma'$ and $\Sigma' - \Sigma''$ should be so small that they might be considered as quantities of a higher order, or should they be $= 0$, the expressions for M$'$ and M$''$ and consequently M will be either too unsafe for calculation or perfectly indeterminate, and the possible errors of observation as well as the term neglected in M might obtain such a preponderating influence that the use of an expression, otherwise very approximate, might lead to essential errors. At least the accuracy of the approximate expression of M, the neglected factor M$''$ being now of the order -1, would be of an order less high by one.

This case of exception will, however, as the view of the formulæ proves, only take place when the three geocentric places of the comet lie apparently in a great circle with the middle place of the sun, or too nearly approach to such a position. It would, however, be wrong to consider this as a case of exception to Olbers's method, as it is founded in the nature of the problem and would and must take place in like manner, in every method (not excepting that of Laplace). Every method which makes use of the important condition of a motion in a plane passing through the sun is liable to it, and must be so, although it is not always mentioned; and every method which neglects this important and simple condition and yet applies no more data of observation, making consequently the problem perfectly determinate, will on that account make a sacrifice in point of accuracy. If we leave for a moment the motion of the earth out of the question and put $\Theta = \Theta' = \Theta''$, the numerator and denominator of M are factors of the projections of $[r\,r']$ and $[r'\,r'']$ on a plane which is perpendicular to R$'$. If these become $= 0$, or too small, the conclusions which may otherwise be derived from their ratio to one another, joined with the condition of a constant plane of motion, as to the distances of the comet from the earth, loses entirely or partly its force, and one is obliged to introduce another, although hitherto unknown quantity, perhaps the distance of the comet from the sun, in order to determine the ratio of the distances with equal accuracy. This is indeed the man-

ner of proceeding of the other methods as well as of Olbers's. The method, as far as it consists not so much in the single formulæ as in the manner of proceeding to find from the ratio of the distances the distances themselves by means of Lambert's problem, is not changed by this case of exception, but the first approximation becomes indeed less accurate, and the calculation becomes somewhat longer.

In this case the combination of the two last equations of (13) cannot be applied, and it will be necessary to make use of the two first alone, or in conjunction with the fourth. If we choose first the second, in which ρ' is already wanting, we may write it thus,

$$\rho'' = \frac{[r' r'']}{[r r']} \cdot \frac{\sin(\alpha' - \alpha)}{\sin(\alpha'' - \alpha')} \cdot \rho$$

$$+ \frac{[r' r''] \, \mathrm{R} \sin(\Theta - \alpha') - [r r''] \, \mathrm{R}' \sin(\Theta' - \alpha') + [r r'] \, \mathrm{R}'' \sin(\Theta'' - \alpha')}{[r r'] \sin(\alpha'' - \alpha')}.$$

The factors $[r' r'']$, $[r r'']$, $[r r']$ contained in the numerator of the latter expression may be considered as the coordinates of the sun in reference to a line of abscissæ, the direction of which is given by α', and may therefore be designated by Y, Y$'$, Y$''$. We have next, accurately to quantities of the second order inclusive,

$$\frac{[r' r'']}{[r r']} = \frac{[\mathrm{R}' \mathrm{R}'']}{[\mathrm{R} \mathrm{R}']} \left\{ 1 - \tfrac{1}{6} (\tau^2 - \tau''^2) \left(\frac{1}{r'^3} - \frac{1}{\mathrm{R}^3} \right) \right\}$$

$$\frac{[r r'']}{[r r']} = \frac{[\mathrm{R} \mathrm{R}'']}{[\mathrm{R} \mathrm{R}']} \left\{ 1 - \tfrac{1}{6} (\tau'^2 - \tau''^2) \left(\frac{1}{r'^3} - \frac{1}{\mathrm{R}^3} \right) \right\}$$

and

$$\frac{[r r']}{[r r']} = \frac{[\mathrm{R} \mathrm{R}']}{[\mathrm{R} \mathrm{R}']}.$$

By the substitution of this value in the latter part, there will appear in the numerator an expression which may be written thus:

$$[\mathrm{R}' \mathrm{R}''] \, \mathrm{Y} - [\mathrm{R} \mathrm{R}''] \, \mathrm{Y}' + [\mathrm{R} \mathrm{R}'] \, \mathrm{Y}''.$$

Now this expression, in consequence of the earth's orbit being a plane, is by (12) = 0. There will remain consequently only the terms multiplied by $\left(\frac{1}{r'^3} - \frac{1}{\mathrm{R}^3} \right)$. If we substitute in these terms for $\frac{[\mathrm{R}' \mathrm{R}'']}{[\mathrm{R} \mathrm{R}']}$, $\frac{[\mathrm{R} \mathrm{R}'']}{[\mathrm{R} \mathrm{R}']}$, their respective approximate values $\frac{\tau}{\tau''}$, $\frac{\tau'}{\tau}$, and introduce Y$'$ instead of Y,

neglecting the higher terms dependent on the interval of time, the formula will assume this shape:

$$\rho'' = \frac{t''-t'}{t'-t} \cdot \frac{\sin(\alpha'-\alpha)}{\sin(\alpha''-\alpha')}\rho - \tfrac{1}{2}\tau\tau' \frac{\sin(\alpha'-\Theta')}{\sin(\alpha''-\alpha')} \cdot \left(\frac{1}{r'^3} - \frac{1}{R'^3}\right) R'.$$

A form quite similar will be obtained by combining the first with the four last of the equations (13). Eliminating from them ρ' we have

$$\rho'' = \frac{[r'r'']}{[rr']} \cdot \frac{\tan\delta'\cos(\alpha-\Theta') - \tan\delta\cos(\alpha'-\Theta')}{\tan\delta''\cos(\alpha'-\Theta') - \tan\delta'\cos(\alpha''-\Theta')}\rho$$

$$-\frac{[r'r'']\,R\tan\delta'\cos(\Theta-\Theta') - [rr'']\,R'\tan\delta' + [rr']\,R''\tan\delta'\cos(\Theta''-\Theta')}{[rr']\,(\tan\delta''\cos(\alpha'-\Theta') - \tan\delta'\cos(\alpha''-\Theta'))}$$

As we before introduced the Y, so we may here introduce the X in reference to the direction Θ', and by the same process we shall obtain

$$\rho'' = \frac{t''-t'}{t'-t} \cdot \frac{\tan\delta'\cos(\alpha-\Theta') - \tan\delta\cos(\alpha'-\Theta')}{\tan\delta''\cos(\alpha'-\Theta') - \tan\delta'\cos(\alpha''-\Theta')}\rho$$

$$-\tfrac{1}{2}\tau\tau' \frac{R'\tan\delta'}{\tan\delta''\cos(\alpha'-\Theta') - \tan\delta'\cos(\alpha''-\Theta')}\left(\frac{1}{r'^3} - \frac{1}{R'^3}\right).$$

Both equations are correct to quantities of the first order inclusive. Their combination gives, by eliminating the term which has the factor $\left(\frac{1}{r'^3} - \frac{1}{R'^3}\right)$, the equation (16).

The proceeding, which in the above-mentioned case has hitherto appeared to me the most convenient, and which has in practice proved to be so as much as could be expected, (it being by almost two orders less accurate than the original formula for M,) consists, therefore, in the following: If the value of M appears to be too indeterminate, on account of the smallness of its numerator and denominator, one may choose in place of it either

$$\text{(28)} \quad \begin{aligned} M &= \frac{t''-t'}{t'-t} \cdot \frac{\sin(\alpha'-\alpha)}{\sin(\alpha''-\alpha')}, \quad \text{or} \\ M &= \frac{t''-t'}{t'-t} \cdot \frac{\tan\delta'\cos(\alpha-\Theta') - \tan\delta\cos(\alpha'-\Theta')}{\tan\delta''\cos(\alpha'-\Theta') - \tan\delta'\cos(\alpha''-\Theta')}, \end{aligned}$$

according as the differences of geocentric longitude or latitude are the more considerable, and consequently allow less influence to the possible errors of observation. The error hereby introduced is of the first order of the intervals of time. The calculation is then carried on to the completion of the trials, by which approximate values of r and r'' become known entirely by formulæ (I) and (II). The value of r' is then found

by interpolating for the interval of time; and denoting the approximate value of ρ thus found by (ρ), the above values of M are to be respectively multiplied by one of these quantities,

$$(29)\quad \begin{cases} \left\{1-\tfrac{1}{2}\tau''\tau'\dfrac{\sin(\alpha'-\Theta')}{\sin(\alpha'-\alpha)}\cdot\dfrac{R'}{(\rho)}\left(\dfrac{1}{r'^3}-\dfrac{1}{R'^3}\right)\right\} \\ \left\{1-\tfrac{1}{2}\tau''\tau'\dfrac{\tan\delta'}{\tan\delta'\cos(\alpha-\Theta')-\tan\delta\cos(\alpha'-\Theta')}\cdot\dfrac{R'}{(\rho)}\left(\dfrac{1}{r'^3}-\dfrac{1}{R'^3}\right)\right\}, \end{cases}$$

accordingly as the first or second formula has been used. The error of this new value of M will generally be of the second order of the intervals of time; and with this value the calculation may in most cases be completed, as, besides, in such a distribution of the observations the accuracy of the determination of the orbit is somewhat more restricted than in the cases in which the more direct process can be adopted.

As an example of approximation I will apply the formula to the above observations. The two first expressions of M will be here

$$\log M = 9{\cdot}70341 \quad\text{and}\quad 9{\cdot}73397,$$

the latter of which, on account of the great change in latitude, would be preferable. Taking now for r' and (ρ) their correct values, viz.

$$\log r' = 0{\cdot}12401$$
$$\log(\rho) = 9{\cdot}80364,$$

(because although the values calculated by the erroneous hypothesis for M would indeed deviate, yet not so much as to sensibly alter the result), the logarithms of the correcting factors will be

$$0{\cdot}05522 \quad\text{and}\quad 0{\cdot}02427,$$

or the corrected values of M,

$$\log M = 9{\cdot}75863 \quad\text{and}\quad 9{\cdot}75824,$$

while the correct expression, as found above, is

$$\log M = 9{\cdot}75799.$$

The difference is inconsiderable enough to be neglected altogether in the case of observations so unfavourably situated.

[To be continued.]

XVI. *Reviews, and Notices respecting New Books.*

Illustrations of the Botany and other branches of the Natural History of the Himalayan Mountains, and of the Flora of Cashmere. By J. Forbes Royle, Esq., F.L.S. F.G.S. &c.

At a time when title-pages often promise so much more than the reader is unfortunately capable of discovering in the works to which they are prefixed, it is refreshing to take up a volume

which under an unassuming head presents us with a mass of valuable information. Under the simple name of 'Illustrations' Mr. Royle has not only afforded us that information which the title of his work would lead us more immediately to expect, but he has also presented us with a comprehensive sketch of the physical features of India in general, and of the Himalayan Mountains in particular; with most valuable details respecting the adaptation of animal and vegetable life to the conditions under which it exists, the distribution of such life according to the localities fitted for it; and also with a variety of other subjects of general interest, which certainly no one would have anticipated from the unassuming title he has given to his work.

It is with much pleasure we observe that Mr. Royle omits few opportunities of illustrating the power of science to contribute to the happiness and welfare of mankind; we particularly allude to those passages where he points out the capabilities afforded by the variously situated portions of our Indian possessions for the growth or cultivation of numerous important productions. We experience this pleasure not because we would wish to degrade science and make it subordinate to the mere acquirement of pounds, shillings and pence, but because it is most desirable to prove to the mass of mankind, that, while their intellectual enjoyments are increased, and their minds are expanded by science, their more ordinary occupations are benefited in proportion to its progress.

Respecting the actual products of India, and its capabilities, Mr. Royle remarks, that

"In the peninsula of India, and in the neighbouring island of Ceylon, we have a climate capable of producing cinnamon, cassia, pepper, and cardamoms. The coffee grown on the Malabar coast is of so superior a quality as to be taken to Arabia and re-exported as Mocha coffee; the Tinnevelly senna brings the highest price in the London market; and there is little doubt that many other valuable products of tropical countries might be acclimated, particularly as several are already in a flourishing condition in the Botanic garden at Calcutta, such as the cocoa and nutmeg, as well as the camphor, pimento and cashew-nut trees. In the Neelgherries a favourable site might without doubt be found for the cinchona, as well as the different kinds of ipecacuanha." . . . . . . Along the coasts of the Bay of Bengal the cocoa and areca-nut palms flourish and abound, and the continent everywhere produces indigo, cotton, tobacco, sugar, and opium. The first, hardly of any note as an Indian product thirty years ago, is now imported in the largest quantities into England. . . . . . . If from these we turn our attention to other products, we shall still see that there are great capabilities even where we should least expect them: for though India is generally looked upon chiefly as a rice country, wheat has been imported into and sold at a profit in England from the northern provinces, and flour for making starch is now one of the annual exports from Calcutta. Of dyes, medicinal drugs, resins, and gums, there are great varieties, and more might be successfully intro-

duced. . . . . . The northern provinces and the hills forming the more immediate object of this work, will be more fully considered in the sequel; it is sufficient at present to observe, that at one season they grow European grains, and at another those which are peculiar to the tropics: and that many perennials of both these two climates seem to succeed equally well in the northern provinces of India; here, therefore, many of the useful products of Persia, Arabia, and Barbary, might be grown. . . . . . Somewhere in the valleys at the foot of the hills (Himalayan), or at moderate elevations, the more generally useful productions of European countries might be successfully introduced, as the olive and hop. . . . . . Here also, as I have recommended in a report to Government, there is considerable prospect of success in the cultivation of the tea-plant, for the different elevations allow of every variety of climate being selected, and the geographical distribution of this plant is sufficiently extended to warrant its being beneficially cultivated." (pp. 4, 5.)

It is clear that the ability of successfully cultivating the plants of some countries, in others where they are not indigenous, will depend upon a knowledge of the climate and other necessary conditions both in the country where the plants are indigenous, and in those to which they are to be transported. At first sight it may appear that sufficient knowledge for the purpose requires very little aid from science: in practice, however, it is found that experiments of this kind frequently fail from inattention to circumstances which to a common observer appear of little importance, but which, when considered by a scientific man, aware of those on which the experiment depends, are probably among the first subjects to which he directs his attention. Hence the value of those researches to ascertain the causes upon which the growth of certain families or genera of plants depends, and of inquiries into the natural distribution of such families or genera over the face of the globe. Mr. Royle's work abounds with valuable information on these heads, more particularly with regard to the vegetation of the Himalayan mountains, and therefore his suggestions for cultivating the productions of other countries within our Indian possessions acquire a value which they could not otherwise possess. It appears also that while other countries may be compelled to contribute a portion of their vegetation for the advantange of India, India is in return forced to send many of her plants for the comfort or gratification of man into various other countries, even into our own northern climate; for our author states, that

"Many of the trees and shrubs of Northern India are now flourishing *in the open air* of England, especially in the gardens of the Horticultural Society of London, and of the Messrs. Loddiges, where may be seen: *Pinus Deodara*; *Webbiana excelsa*, *Gerardiana* and *Morinda*; *Rhododendron arboreum*, *aristatum*, *campanulatum* and *lepidotum*; *Pyrus vestita* and *variolosa*; *Cratægus glauca*; *Cotoneaster microphylla*, *rotundifolia*, and several other species; *Benthamia fragifera*; *Rosa sericea*, *macrophylla*, and *Brunonis*;

Berberis Asiatica, aristata and *Wallichiana*; *Potentilla atrosanguinea* and *Nepalensis*; *Salvia nubicola*, and many others." (p. 42.)

When we search into the present distribution of animal and vegetable life over the face of the globe, there are few subjects of greater interest than the correspondence or difference, as the case may be, between the animal or vegetable forms which are found under nearly equal climates, particularly as regards temperature, whether such equal temperatures be obtained by variations in altitude above the level of the sea in different latitudes, or by the equal approximations of localities to the equator or to the poles, as may happen. The Himalayan mountains rising to a great height in a warm latitude, and bounding the lower lands of India on the northward, necessarily afford excellent opportunities for studying the effects of diminishing temperature on animal and vegetable life as the traveller proceeds from the heated plains at the southern base of these mountains, and rises to the line of perpetual snow of the highest range. These opportunities were not likely to be overlooked or lost by a person of Mr. Royle's acquirements, and accordingly those parts of his work which are published, contain some very remarkable facts connected with this subject, and it is understood that the parts which are to follow will afford us important additions to them.

Mr. Royle divides the slopes of the Himalayan mountains into three belts, the first may be considered to reach "to between four and five thousand feet of elevation, as several tropical perennials extend to the latter, and snow does not usually fall below the former. The second belt may be conceived to embrace the space between five and nine thousand feet of elevation, as the winter's snow is always melted from such elevations before the accession of the rainy season, and the upper is nearly the limit to which the herbaceous plants of tropical genera extend. The third belt may be taken from this elevation up to the highest limits from which snow melts away on the southern face of the Himalayan mountains." (p. 16.) Among the difficulties noticed by the author which attend this division of the slopes of these mountains into belts of vegetation, he adduces that "produced by the great difference in the vegetation of the northern and southern faces of the very same range or mountain, so that frequently a straight line running along the summit of the ridge may be seen dividing the luxuriant arboreous and shrubby vegetation of the northern face from the brown, barren, and grassy covering of the southern slope." (p. 16.)

Mr. Royle offers the following explanation of the remarkable fact, that the limit of the line of snow rises higher on the northern than on the southern flank of the Himalayas.

"From the details which have been given, it seems abundantly clear that the elevation of the Indian snowy range is sufficient to prevent the passage across of the cloudy masses which deluge the plains of Northern India with rain, both in the cold and in the warm season. The atmosphere, therefore, on the northern face of the Himalayas preserves unimpaired the dryness, which is the characteristic of the rarefied air of lofty situations: hence the little

deposition of snow which takes place in winter in proportion to the lowness of the temperature. The returning warmth of spring rapidly dissolves this thin layer of snow from level places, in consequence, it appears, of the undiminished power of the solar rays in passing through so rare and transparent a medium; a fact tending to confirm Mr. Daniel's views respecting the superior energy of the solar rays in the higher regions of the air; and as this seems already to have been done with respect to his opinions of their great power in polar regions, the fact is interesting as giving an additional cause for the analogy between alpine and polar vegetation. When the snow is once melted, these elevated tracts, surrounded and confined by towering mountains, absorb heat as readily during the presence of the sun, as they radiate it freely while he is absent; and becoming, like the surface of the earth at ordinary levels, the source whence the heat received from the sun is diffused to surrounding objects, they cause the line of perpetual congelation to recede higher and higher in proportion to their own elevation. Peaks and pinnacles, on the contrary, projected into the air like promontories into the ocean, partake rather of the equability of temperature of the media into which they intrude, than impress on them, like plains and table lands, their own extremes of heat and cold." (p. 39.)

Mr. Royle's work is illustrated by numerous well-executed plates, more especially of plants, and by geological sections which are to be described in the forthcoming numbers. Altogether, we highly recommend this publication to our readers, containing as it does, not only an ample store of information respecting the natural productions of the Himalayas, but also the best general view of the physical features of those magnificent mountains with which we are acquainted.

XVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. vi. p. 375.]

April 2.—A PAPER was read, entitled, "On the Results of Tide Observations, made in June 1834, at the Coast-Guard Stations in Great Britain and Ireland." By the Rev. W. Whewell, F.R.S., Fellow of Trinity College, Cambridge.

On a representation made by the author of the advantages which would result from a series of simultaneous observations of the tides, continued for a fortnight, along a great extent of coast, orders were given for carrying this measure into effect at all the stations of the Preventive service on the coasts of England, Scotland, and Ireland, from the 7th to the 22nd of June inclusive. From an examination of the registers of these observations, which were transmitted to the Admiralty, but part of which only have as yet been reduced, the author has been enabled to deduce many important inferences. He finds, in the first place, that the tides in question are not affected by any general irregularity, having its origin in a distant source, but only by

such causes as are merely local, and that therefore the tides admit of exact determination, with the aid of local meteorological corrections. The curves expressing the times of high water, with relation to those of the moon's transit, present a very satisfactory agreement with theory; the ordinates having, for a space corresponding to a fortnight, a minimum and maximum magnitude, though not symmetrical in their curvatures on the two sides of these extreme magnitudes. The amount of flexure is not the same at different places; thus confirming the result already obtained by the comparison of previous observations, and especially those made at Brest; and demonstrating the futility of all attempts to deduce the mass of the moon from the phenomena of the tides, or to correct the tables of the tides by means of the mass of the moon. By the introduction of a local, in addition to the general, semimenstrual inequality, we may succeed in reconciling the discrepancies of the curve which represents this inequality for different places; discrepancies which have hitherto been a source of much perplexity. These differences in the semimenstrual inequality are shown by the author to be consequences of peculiar local circumstances, such as the particular form of the coast, the distance which the tide wave has travelled over, and the meeting of tides proceeding in different directions; and he traces the influence of each of these several causes in producing these differences. A diurnal difference in the height of the tides manifests itself with remarkable constancy along a large portion of the coast under consideration. The tide hour appears to vary rapidly in rounding the main promontories of the coast, and very slowly in passing along the shores of the intervening bays; so that the cotidal lines are brought close together in the former cases, and, in the latter, run along nearly parallel to the shore; circumstances which will also account for comparative differences of level, and of corresponding velocities in the tide stream. The author intends to prosecute the subject when the whole of the returns of these observations shall have undergone reduction.

A paper was also read, entitled, "Copies of Registers of the Thermometer kept at Alford, Aberdeenshire." By the Rev. James Farquharson, F.R.S.

The observations recorded in these tables were made at $9^h\ 15^m$ A.M., and at $8^h\ 30^m$ P.M., each day of the year 1833; and the highest and lowest temperatures in each month observed from the indications of Six's thermometer. The author remarks that the differences between the temperature of the morning and evening hours of observation were greatest, on an average, during clear weather; that is, when the radiation of heat from the ground is greatest.

The reading of another paper, by the same author, entitled "On the Ice, formed under peculiar circumstances, at the bottom of running Water," was commenced, but not concluded.

April 9.—The reading of a paper entitled, "On the Ice, formed under peculiar circumstances, at the bottom of running Water." By the Rev. James Farquharson, of Alford, F.R.S., was resumed and concluded.

The ice which is frequently observed to collect at the bottom of

streams and rivers, differs in appearance from that which is formed at the surface; for, instead of assuming the shape of solid glass-like plates, it has more the appearance of aggregated masses of snow, and is composed of small crystals of ice adhering together irregularly, either by their sides or angles. Rivers are sometimes so choked up by accumulations of ground-ice of this description, that they are not only impeded in their course, but also raised considerably above their banks. While in this state, a slight change in the weather will frequently occasion the complete disengagement of this ice from the bottom; so that, in a very short space of time, the river returns into its natural channel; and then, although it may be frozen at the surface, it continues to flow over a perfectly clear bottom. All these phænomena are considered by the author as perfectly explicable on the theory he advances, of different degrees of radiation of heat occurring from the bottom according to variations of circumstances. He conceives that when this radiation takes place from the solid opake materials of the bed of the stream, through the stratum of transparent water, congelation is induced on that portion of fluid, already cooled down to the freezing-point, which is in immediate contact with the radiating body. The circumstances which, by favouring radiation, contribute to this effect, are, principally, great clearness of the sky, and great transparency of the water; the bottom of the river being cooled below the freezing-point sooner than the water which is flowing over it; and the ice, formed at the bottom, remaining attached to it, as long as the heat which is transmitted from below continues to be lost by radiation. The formation of ground-ice is favoured by the intestine motions incident to a rapid current; because the different strata of fluid, which in still water would have arranged themselves, according to their specific gravities, in the order most conducive to the congelation of the surface, being continually mixed together, the whole body of water is cooled more uniformly.

The Society then adjourned over the Easter recess to meet again on the 30th instant.

April 30.—A paper was read, entitled, "Continuation of the paper on the relations between the Nerves of Motion and of Sensation, and the Brain; and more particularly on the structure of the Medulla Oblongata and of the Spinal Marrow." By Sir Charles Bell, F.R.S.

The author enters into a minute anatomical investigation of the structure of the spinal cord, and of its relations with the encephalon, and with the origins of the nerves. He finds that the spinal cord is constituted, in its whole length, by six pairs of columns, namely, two posterior, two lateral, and two anterior; each column being composed of concentric layers, and invested with an external coating of cineritious substance, and all the columns being divided from each other by deep sulci, which penetrate nearly to the centre of the cord. On tracing the posterior columns in their ascent towards the encephalon, they are seen to diverge laterally at the *calamus scriptorius*, or bottom of the fourth ventricle, and to proceed into the substance of the cerebellum. Each of these posterior columns is here found to consist of two portions, the outermost being the largest; and they now consti-

tute the *processus cerebelli ad medullam oblongatam.* This subdivision of the posterior columns may be traced throughout the whole length of the spinal cord. The lateral columns give origin to the posterior roots of the spinal nerves, and are therefore the parts subservient to sensation. In ascending towards the brain, each of these columns has a double termination; first, in the root of the fifth pair of cephalic nerves; and secondly, in the place where both columns unite into one round cord, and mutually decussate.

Between the lateral and the anterior columns there is interposed a layer of cineritious matter, constituting a continuous stratum from the *cauda equina* to the roots of the auditory nerves. There is also a septum, dividing the right and left tracts subservient to sensation in the region of the fourth ventricle, and apparently terminating at the point of decussation of these tracts; but, in reality, separating to allow of this decussation, and joining the central portion of the cord, which connects the posterior with the anterior columns, and extends from the *pons Varolii* to the *cauda equina.*

The anterior columns, constituting, at their upper part, the *corpora pyramidalia,* after their union and decussation, compose the motor columns of the spinal cord. They do not, in their course, unite or decussate with the lateral, or sensitive columns; decussation taking place only among the columns performing similar functions; that is, the motor columns with the motor, and the sensitive with the sensitive.

May 7.—The first paper read was entitled, "On the Elements of the Orbit of the Comet of Halley in 1759." By J. W. Lubbock, Esq., V.P. and Treasurer of the Royal Society.

In calculating the elements of Halley's comet, former astronomers have in general adopted the parabolic hypothesis, neglecting the reciprocal of the semi-axis major; and even in the more recent investigations of its orbit, no accurate value of this quantity has been employed. Mr. Lubbock, perceiving the serious effect which an error in the semi-axis major would occasion in the determination of the other elements, renewed these very laborious calculations, assuming as the value of this quantity that given by M. Pontécoulant, in his "Théorie analytique du Système du Monde;" taking also into account the alterations which the elements of the comet have undergone by the action of the planets, and likewise the effect of precession upon the longitude of the node, and of the perihelion. The author takes this opportunity of correcting the very erroneous statements that have been made respecting the results of his investigations, especially with regard to the time of the perihelion passage, which is, of course, very different from that of its actual appearance to spectators on the earth; although these two epochs are frequently confounded with one another.

The second was entitled, "Formulæ for computing the Longitude at Sea;" by William Dunlop, Esq. Communicated by the Secretaries.

These formulæ, in which the longitude and latitude of two points in a spherical surface, together with the arc of the great circle intercepted between them, are supposed to be given, furnish the means

of determining the longitude of any other point in that circle, from its latitude.

The third paper was entitled, " Hygrometrical Observations made on board His Majesty's surveying vessel Ætna." Communicated by Captain Beaufort, R.N., F.R.S.

These observations extend from the 27th of March to the 6th of July, 1834, and were made daily at 8 o'clock A.M., at noon, and at 4 o'clock P.M. They comprise the height of the barometer, the dew-point, degrees of dryness on the thermometrical, and of moisture on the hygrometrical scales, the elasticity of the vapour, and the number of grains of vapour in a cubic foot; with occasional remarks. A second series is also given, exhibiting the progress of solar radiation.

The fourth was a " Meteorological Register, from the 1st of January to the 1st of November, 1834," by Mr. Edward Barnett. Communicated by Capt. Beaufort, R.N., F.R.S.

These observations, made during a voyage across the Atlantic, relate chiefly to the temperatures of the air and of the surface of the sea.

The fifth was a " Meteorological Register, kept on board His Majesty's Ship Thunder, between the 1st of January and the 30th of June, 1834," by R. Owen, Commander. Communicated by Captain Beaufort, R.N., F.R.S.

These observations relate to the state of the weather, the direction and force of the wind, and the heights of the thermometer, and of the marine and oil barometers.

May 14.—A paper was read, entitled, " An Account of the Water of the Well *Zem-zem,* with a qualitative analysis of the same by Professor Faraday"; in a letter from John Davidson, Esq., to the Secretaries, and communicated by them.

The author having, during his stay at Jedda, the port of Mecca, succeeded in procuring about three quarts of the water from the well of *Zem-zem,* to which the Mahomedans ascribe a sacred character and extraordinary virtues; and wishing to preserve this water for the purposes of analysis, had the can in which it was contained carefully sealed; but, unfortunately, on its arrival in the London Docks, the can, notwithstanding the directions written on it, was opened, and the gas with which it was highly charged, and by which it held in solution a very large quantity of iron and other matters, was allowed to escape. The precipitate thrown down, in consequence of the loss of this gas, was found, by Professor Faraday, to consist of carbonate of protoxide of iron in the enormous proportion of 100·8 grains to the imperial pint of water. The clear fluid was neutral, and contained much muriate, and a little sulphate, but no carbonate; together with a little lime, potash, and soda. There was also found an alkaline nitrate in considerable quantity; this Mr. Faraday conjectures to have been saltpetre, which had been added to the water by the priests.

The reading of a paper was commenced, entitled, " Observations on the Theory of Respiration." By William Stevens, M.D., D.C.L., Fellow of the Royal College of Physicians of Copenhagen, and of Surgeons of London. Communicated by W. T. Brande, Esq., V.P.R.S.

May 21.—The reading of the paper, entitled, "Observations on the Theory of Respiration." By William Stevens, M.D., D.C.L., Fellow of the Royal College of Physicians of Copenhagen, and of Surgeons of London. Communicated by W. T. Brande, Esq., V.P.R.S., was resumed and concluded.

From the fact that no carbonic acid gas is given out by venous blood when that fluid is subjected to the action of the air-pump, former experimentalists had inferred that this blood contains no carbonic acid. The author of the present paper contends that this is an erroneous inference; first, by showing that serum, which had been made to absorb a considerable quantity of this gas, does not yield it upon the removal of the atmospheric pressure; and next, by adducing several experiments in proof of the strong attraction exerted on carbonic acid both by hydrogen and by oxygen gases, which were found to absorb it readily through the medium of moistened membrane*. By means of a peculiar apparatus, consisting of a double-necked bottle, to which a set of bent tubes were adapted, he ascertained that venous blood, agitated with pure hydrogen gas, and allowed to remain for an hour in contact with it, imparts to that gas a considerable quantity of carbonic acid. The same result had, indeed, been obtained, in a former experiment, by the simple application of heat to venous blood confined under hydrogen gas; but on account of the possible chemical agency of heat, the inference drawn from that experiment is less conclusive than from experiments in which the air-pump alone is employed. The author found that, in like manner, atmospheric air, by remaining, for a sufficient time, in contact with venous blood, on the application of the air-pump, acquires carbonic acid. The hypothesis that the carbon of the blood attracts the oxygen of the air into the fluid, and there combines with it, and that the carbonic acid thus formed is afterwards exhaled, appears to be inconsistent with the fact that all acids, and carbonic acid more especially, impart to the blood a black colour; whereas the immediate effect of exposing venous blood to atmospheric air, or to oxygen gas, is a change of colour from a dark to a bright scarlet, implying its conversion from the venous to the arterial character: hence the author infers that the acid is not formed during the experiment in question, but already exists in the venous blood, and is extracted from it by the atmospheric air. Similar experiments made with oxygen gas, in place of atmospheric air, were attended with the like results, but in a more striking degree; and tend therefore to corroborate the views entertained by the author of the theory of respiration. According to these views, it is neither in the lungs, nor generally in the course of the circulation, but only during its passage through the capillary system of vessels, that the blood undergoes the change from arterial to venous; a change consisting in the formation of carbonic acid, by the addition of particles of carbon derived from the solid textures of the body, and which had combined with the oxygen supplied by the arterial blood: and it is by this combination that

[* See Prof. Graham on the law of the diffusion of gases, Lond. and Edinb. Phil. Mag., vol. ii. p. 354.—Edit.]

heat is evolved, as well as a dark colour imparted to the blood. The author ascribes, however, the bright red colour of arterial blood, not to the action of oxygen, which is of itself completely inert as a colouring agent, but to that of the saline ingredients naturally contained in healthy blood. On arriving at the lungs, the first change induced on the blood is effected by the oxygen of the atmospheric air, and consists in the removal of the carbonic acid, which had been the source of the dark colour of the venous blood; and the second consists in the attraction by the blood of a portion of oxygen, which it absorbs from the air, and which takes the place of the carbonic acid. The peculiar texture of the lungs, and the elevation of temperature in warm-blooded animals, concur in promoting the rapid production of these changes.

May 28.—A paper was in part read, entitled, "On the Influence of the Tricuspid Valve of the Heart on the Circulation of the Blood." By Thomas Wilkinson King, Esq., M.R.C.S. Communicated by Thomas Bell, Esq., F.R.S.

GEOLOGICAL SOCIETY.

(*Address of the President, G. B. Greenough, Esq., F.R.S., at the Annual Meeting, 20th February,—continued.*)

Gentlemen, we had many of us an opportunity of witnessing at the late Meeting of the British Association the increasing interest and success with which geology is pursued in Scotland, and we felt more especially grateful on that occasion to Lord Greenock and the Highland Society, for the exertions which they have recently made to unravel the structure of their native land, and more especially the nature of its coal-fields. It is not my intention to detail to you all the proceedings of that Society, but I must not refrain from attributing mainly, if not solely, to their exertions the provision which the Government have lately made for the immediate publication of Dr. MacCulloch's geological map of Scotland. Whatever may be the intrinsic excellence of that work, it must be eminently useful, if considered only as a nucleus, round which will immediately congregate those ample stores of geological knowledge which at present lie latent in the minds and cabinets of our northern brethren. Nor will Ireland be backward in furnishing her contingent. The coloured copy of Arrowsmith's map of that portion of the United Kingdom which Mr. Griffith has undertaken to lay before the British Association in August next, will bring within our reach an abundant supply of geological information, which though it has been in his possession for many years past, a natural repugnance to combining geological correctness with geographical inaccuracy has hitherto induced him to withhold.

The exertions of the Geological Society of Dublin have been continued, and cannot fail to diffuse over the whole of Ireland a taste for those studies which at a very early period reflected so much lustre on the name of Kirwan.

It will be in your recollection that Mr. Weaver presented to us some time since a valuable Memoir on the Geology of the south-western part of that country. In one part of the Memoir the coal-

measures of the county of Kerry were referred to the transition series; the correctness of this statement was questioned at the time, and various inquiries were instituted and persevered in, without leading however to any very decisive result. Since the commencement of the session, the author on re-examining the district, has with great candour acknowledged himself to have been in error. More diligent investigation brought into view a well-characterized band of old red sandstone, intervening in one part of the coal-field, between the carboniferous and the transition strata.

Mr. Jephson has transmitted to us an account of a remarkable spring at Mallow in the county of Cork, the temperature of which varies from 67° to $71\frac{3}{10}$°. It breaks out in limestone.

An ample and able account of the recent progress of our science on the Continent will be found in the Report of M. Boué to the Geological Society of France. I shall, therefore, confine my observations on this head almost exclusively to the Papers which have been read at our evening meetings.

The first in order relates to the loamy deposit, called in the valley of the Rhine, Loess, a term as yet scarcely naturalized among us, and which, I believe, is correctly represented by the word Silt. This paper, from the pen of Mr. Lyell, has since been published entire in Jameson's Journal.

Intimately connected with this is a communication by Mr. Horner, on the nature and quality of the solid matter actually suspended in the water of the Rhine. To ascertain them the author made experiments during the months of August and November, bringing up about a gallon of water from different depths and drying slowly the solid matter obtained from it. With whatever attention to accuracy such experiments are conducted, they must, I conceive, be multiplied almost indefinitely before we can arrive in safety at any general conclusion upon so intricate a problem*.

From Colonel Silvertop we have received a description of certain tertiary deposits, which in the kingdom of Murcia, in Spain, occupy extensive plains, bounded by discontinuous ridges of nummulitic limestone, transition rocks and mica-slate: the author divides these deposits into four districts, and each of these is separately treated. M. Deshayes refers their imbedded fossils to the second and third deposits of tertiary formation.

In a work on Spain, published during the past year by Captain Cooke, will be found a brief account of the mines and rocks of that hitherto partially examined country.

I may also be permitted to notice among the additions which have been made to our library, an excellent Memoir by M. le Chevalier Albert de la Marmora, on the constitution of the Balearic Islands.

No communication has been made to us from Asia since the last Anniversary.

[* Notices of Mr. Horner's paper will be found in Lond. and Edinb. Phil. Mag., vol. v. p. 211, and vol. vi. p. 396.—Edit.]

A paper by Mr. Cunningham describes the physical structure, and to a certain extent, the geological composition of the country between Hunter's River and Moreton Bay, in Australia, and is accompanied by a valuable map and section and a small collection of rock specimens. The additions made during the expedition referred to by Mr. Cunningham are important, and the geological notices, though slight, will be welcomed by future inquirers.

Mr. Rogers, who laid before the British Association at Edinburgh an able sketch of the "Geology of North America," has more recently favoured this Society with an account of the strata situate on the banks of the Missouri and Mississippi rivers, and further, in the district of the Rocky Mountains. It may be said of all these papers that they are in a great degree compilations, but compilations so executed are perhaps among the most valuable documents that can be transmitted to us. No general views could ever be opened if every author were to confine his descriptions and reasonings to those minute tracts which have fallen within the sphere of his own personal examination. Every system and theory is necessarily founded upon details industriously collected from various quarters.

Besides these communications, we have received from America recently two works, in which the same subject is treated with great clearness and in considerable detail: the one entitled "Contributions to Geology, by Isaac Lea, accompanied by six plates of Shells," of which some at least are not very accurately figured; the other "A Synopsis of the Organic Remains of the Cretaceous Rock, with nineteen lithographed plates of Shells, by Dr. Morton." These works, together with the papers of Mr. Conrad published previously in the American Journal of Science and the Journal of the Academy of Natural Sciences of Philadelphia, illustrated also with lithographic plates, have rendered the upper formations of the United States as intelligible as those of our own country.

Dr. Morton notes the generic accordance of the Testaceous Mollusca on the east and west shores of the Atlantic; but independently of genera, there are at least twenty-four species common to both. In like manner some identities have been traced in the tertiary deposits of Europe and America. The *Pecten quinquecostatus* in particular occurs equally in the cretaceous group on both sides the Atlantic; nor is the analogy confined to Testacea; it extends to the Saurian reptiles. The animals whose remains are found in chalk formerly inhabited the seas of the two continents, and whatever cause bared the eastern, appears to have acted simultaneously on the western mass; not a rush of currents, but a subsidence or elevation.

In the county of Onondago, in New York, is a lacustrine deposit still forming, in which thousands of tons might be obtained of bleached shells. The shells at the mouth of the Potomac river, belonging to the newer Pliocene beds, retain their colours; twenty-nine of the species are the same with those which now live, and of these there are seven only which are not known to inhabit the coast of America.

From the upper marine deposit of Dr. Morton, which corresponds to the lower tertiary of Mr. T. A. Conrad, and to the older Pliocene of Mr. Lyell, numerous specimens were exhibited to us in the course of last session by Mr. Finch *. Of fifty-six species of shells observed by Mr. Conrad in this deposit, which extends through Maryland, Virginia, and the county of Cumberland, in New Jersey, one th rd still exist on the coast of America, but some species in a more southern latitude than that in which they are found fossil.

The Miocene beds, if they occur, have hitherto escaped detection. The Eocene, the middle tertiary of Mr. Conrad, which in England is known as the London clay, and in France as coarse limestone, assumes in America the character of siliceous sand, and in that form has been traced in a north-eastern and south-western direction from Alabama, through South Carolina, Georgia, and Florida, as far as the Gulf of Mexico. Two hundred and nineteen species of shells found in this deposit have been described by Mr. Lea, but among them all, there is perhaps not one entirely analogous to any living species. Several of these shells belong to genera unknown upon the coast of America, some to genera found fossil in Europe, some to genera entirely new. It may be doubted whether any of the species correspond with any of the Eocene fossils of Europe, but the number of turreted shells and generic resemblance satisfactorily establish the epoch to which they belong.

It appears from the observations of M. Dufrénoy that in the chalk of the Pyrenees fifty species, in a list of about two hundred, have the character of tertiary shells. A corresponding gradation in the fossil contents of the tertiary and cretaceous formations is observable in America. The Chalk, or rather the Chalk-Marl, of the new continent occupies large tracts in New Jersey, Delaware, and Alabama, and contains among other organic remains teeth of the Mosasaurus, in no respect differing from those collected at Maestricht.

Mr. Rogers recovers the Chalk formation on the banks of the Missouri, and about the mouth of the Omawhaw; its transverse limit is not known. No flints appear in the beds, but flint nodules, like the English, occur plentifully lower down the river, even to the Mississippi.

The Ferruginous Sand of America reposes in the northern states of the Union as in Sweden and along the Carpathian mountains, upon primary rocks; in the southern, upon limestone, perhaps our mountain limestone; it forms an irregular crescent, extending nearly three thousand miles, through Jersey, Delaware, Maryland, Virginia, the two Carolinas, Georgia, Alabama, Mississippi, Tenessee, Louisiana, Arkansas and Missouri.

Dr. Morton and Mr. Rogers refer this formation to the Green-Sand of England with more confidence perhaps than their observations warrant. Sands red and green occur in Europe both above the chalk and below it. Zoological evidence rather militates against their conclusion. With one or two exceptions all the species are peculiar

[* Mr. Finch's collection has since we believe been purchased for the British Museum.—Edit.]

to the western continent. *Pecten quinquecostatus*, the only shell which is quoted as being common to the sands of the United States and this country, occurs also at Maestricht, and Baculites are characteristic of the upper part of the chalk. From the occurrence of great quantities of lignite in this formation, from the remains discovered in it of the Scolopax, a bird which inhabits the sea-shore, and from the locality of the beds in reference to the ancient coast line, Mr. Rogers infers that the deposit took place in shallow water, along a coast, which like the present, presented a very extensive range of soundings; to this circumstance he attributes the difference of the American and European species of sea-shells at the same period.

With greater probability, as far as the evidence of fossils is concerned, Mr. Rogers attributes to the Green-Sand Formation of England a deposit traced from below the Big Bend to the Rocky Mountains both on the Missouri and the Yellow River, characterized by Hamites, *Gryphæa Columba*, and *Belemnites compressus*. Above the Big Bend horizontal beds of lignite, sandstone, shale and clay, occur continuously for several days' journey. The author considers this formation to be of more recent birth; it contains, near the Cherry River, beds of lignite from three to nine feet in thickness.

The New Red Sandstone, with its usual accompaniments of sand and gypsum, appears to be in North America developed very extensively. According to Mr. Rogers, it comprehends all the country from the falls of the Platte to the great salt lake, or rather sea, on the western side of the Rocky Mountains, and from the Missouri to the Arkansas and Rio Colorado. The same formation is supposed to extend into Mexico, and to be the red sandstone described by Humboldt as occurring so extensively in the southern provinces.

On ascending the Missouri from its junction with the Mississippi the cliffs are found to consist of Limestone, characterized by Productæ, Terebratulæ and Encrini. The hills near Cheriton are composed of this limestone, and good beds of bituminous coal occur in the same district.

The relative position of the vast deposits of Coal and Anthracite which have been discovered in America is not yet satisfactorily ascertained. The great coal-field of Pennsylvania is said to occur in the higher beds of grauwacké, but what are so called may possibly be shown hereafter to correspond to the limestone shale and millstone grit of Derbyshire. When skilfully treated, this anthracite is considered better than the best bituminous coal of England and the United States. Vegetable impressions are rare, and I do not find that any of the Species have been identified with the English, but the Genera, I believe, are the same. The next great deposit of anthracite, that of Rhode Island, lies rather lower in the series, and the anthracite of Worcester is said to occur in an imperfect mica slate, associated with gneiss. Dr. Meade states, that at Rhode Island the veins of coal are separated by various coloured sandstones of the transition series, yet fine specimens of indurated talc and green asbestus in capillary crystals are also interspersed through the shale, and form the immediate cover of the coal.

The Rocky Mountains, as far as Mr. Rogers could collect from the information of Mr. Sublette, a person engaged for eleven years in the fur trade, and from the journals of Long and Lewis and Clarke and Nuttal, are Primitive. The eastern chain, called the Black Hills, consists of gneiss, mica slate, and greenstone, with amygdaloid and other volcanic substances. Volcanic mounds are frequently seen on the west of the mountains between the rivers Salmon and Louis; for the distance of more than forty miles the Columbia river flows between perpendicular cliffs, from two to three hundred feet in height, composed of lava and obsidian. The Malador branch of the Columbia takes its direction through a similar gorge, and thermal springs abound in this part of the country.

On the various organic remains of North America, a Paper by Dr. Harlan, which first appeared in the Transactions of the Geological Society of Philadelphia, has been republished in Jameson's Journal.

A valuable Communication on the Bermudas, with which we have been favoured by Lieutenant Nelson, R.E., has taught us that in explaining the formation of strata our homage is not exclusively due to Neptune, Vulcan, and Pluto, but that Æolus must also be regarded.

This cluster of islands consists entirely of coral, of what kinds it is unnecessary to specify here, though the author has bestowed upon this part of the subject a large share of attention. Confining myself to what relates more especially to geological science, I may state the following as the most important conclusions which Lieutenant Nelson's observations tend to establish: 1. That the coral animal does not build above water. 2. That coral islands now in process of forming may and do attain a considerable height, say 260 feet above the level of the sea, without the assistance of volcanoes, earthquakes, or any other violent catastrophe. 3. That this height has in Bermuda been attained by a mere accumulation of sand and shells, continually blown up and advancing from the coast into the interior. 4. That drift sand is capable of arranging itself in strata. 5. That of the strata so formed some may be consolidated, others unconsolidated, and that the two may alternate. 6. That strata of drifted sand do not present horizontal surfaces. 7. That wind is capable of giving to strata the figure of a dome or saddle, or a waved and contorted appearance, or an arrangement round centres, or a high degree of inclination. 8. That in coral islands bays are original indentations, not the effects of subsequent abrasion. 9. That the surface of a country may be diversified by hill and dale, though it has never undergone diluvial action. 10. That under favourable circumstances denudation may be occasioned by wind as well as by water. 11. That the ripple-mark, which Mr. Scrope* ascribes to a vibratory movement of the lower stratum of water, agitated by winds or currents, may also be owing to wind. 12. That crevices or fissures may be the results of contraction or unequal expansion,

* Proceedings of the Geological Society, No. xxi. 1831.

and are not necessarily accompanied by violence. 13. That the reticulation of such crevices does not disprove their being contemporaneous. 14. That caves may be produced in strata by the undermining action of the sea. 15. That limestone may be consolidated without the application of either heat or pressure.

The Bermuda Islands furnish a striking example of the intermixture of land and sea shells with the bones of birds and tortoises, and likewise with vegetable remains. Some of the specimens which accompany the paper have a structure distinctly oolitic, and in some I observe the delicate red tint which is met with in the chalk beds of Yorkshire, or the oolite of Dijon. The cause of this, and still more, the origin of the sand, the detritus of rubies which occurs in one part of the shore, are curious subjects of inquiry. It is also remarkable that breccias should be found at Bermuda, similar to those of Nice, the island of Cerigo, and Gibraltar.

A paper on the arrangement of Fossil Fishes, read at the first meeting after the recess, and ably commented upon by its author, M. Agassiz, received from you more than usual marks of approbation. M. Agassiz informed us, that as yet he had not found any species identical with those of our present seas, with the exception of one small fish which has been discovered in Greenland imbedded in geodes of clay, the geological age of which is undetermined. In the newer tertiary formations, viz. the Crag and superior Apennine beds, the species for the most part exhibit a relation to the genera which dwell within the tropics, but in the older tertiary, viz. the London clay, the marine limestone of Paris and the rock of Monte Bolca, at least a third of the fishes belong to genera that are extinct. In the Chalk more than two thirds belong to extinct genera, and if the grouping of strata were regulated only by ichthyological considerations, this rock would be more properly classed with the tertiary formations than the secondary. Below the Chalk not one recent species has been met with; in the Wealden Beds, the Oolitic Beds, and the Lias, even the genera are all different from those in the chalk. Below the lias, two out of the four orders, under which M. Agassiz comprehends all the fishes that are known, viz. the Cycloidean and the Ctenoidean, entirely vanish, while the other two orders, rare in our days, suddenly appear in great numbers, together with large sauroid and carnivorous fishes. Of the fishes that occupy the Transition Rocks few have been brought to light, and no peculiar character has yet been affixed to them. In general the more ancient fishes are the best protected by scales. Those which are more ancient than the green-sand exhibit none of those marks by which we can determine in the fishes of our own times whether the water in which they live be fresh or salt; the species always changes with the formation, and frequently, as we see, the genus also. It would appear, therefore, that greater changes take place in the higher order of animals than the lower in equal periods of time.

Your award of the Wollaston medal to this eminent naturalist has led to the most advantageous results. By that award M. Agassiz having been induced to come over to this country, has received in all

quarters that distinction which his superior knowledge and personal character and deportment justly deserve. With a view to enable him to devote a larger portion of time to the study of fossil Ichthyology in Great Britain, the Association for the Advancement of Science voted to him at Edinburgh the sum of 100*l*. During his subsequent excursions in various parts of England and Ireland he had ample opportunities of visiting whatever collections have been made in that department of natural history to which he devotes himself; and every one was happy to transmit to our apartments at his request any specimens which he wished to figure. In the very short space of time to which his stay in this country was necessarily confined, M. Agassiz was enabled to add to the very large number of species which he had already examined, no less than two hundred that were entirely new to him; these were placed, immediately as they arrived, in the hands of an artist from Neufchatel, acting under M. Agassiz's direction. Such are the facilities and advantages which Associations like ours supply to those whom our motto designates as true sons of science!

Sir Philip Egerton has drawn out for us a Catalogue of a rich Collection of Specimens formed by himself and Lord Cole in the caves of Franconia and the Hartz Mountains. In the cavern at Galenreuth, now closed against visitors, it was their good fortune to obtain several bones of the fossil bear, which the late Baron Cuvier required to complete the skeleton of that animal. Many of them appear to have been scratched, but none gnawed. In all these caverns, recent bones referrible to various animals, accompany the fossil; and in some of them have been found old coins, iron implements, and fragments of rude pottery.

To a work which I shall have occasion to bring under your notice hereafter, Mr. Broderip, our Vice-President, has appended a Table, showing the situation and depth at which the different genera of shells are found in seas and estuaries. The importance of such a table, though professedly incomplete, must be evident to you all; and I hope we shall receive from the same quarter further proofs of the advantage which our science is capable of receiving by allying itself with practical zoology. One of my predecessors has adverted to various circumstances which may determine different fossils to different localities, producing an abundant supply in one place, and a comparative dearth in another. In this point of view, Mr. Broderip's table will be found of great use. By referring to it we discover at once what genera in the present creation are confined to shallows; what genera are to be expected at depths varying from a few feet to three or four hundred, and even more: which are those that attach themselves to marine plants, drifted wood, coral, crustacea, loose stones, or rocks; perforate the shells of other animals, coral, wood, or arenaceous and calcareous deposits; or dwell on beds of mud or sand. A knowledge of the habits of recent Testacea must materially assist our investigations into the habits of corresponding fossil genera.

The Geographical Range of different fossil Animals is a subject of great interest, coinciding as it must do with the range of those con-

ditions which were essential to their birth and preservation. To Col. Sykes we are greatly indebted for bringing under our notice a Collection of fossils made by Captain Smee in the district of Cutch. On comparing these remains with the fossils of the oolitic series in England, we observe, not without surprise, that one agrees in every respect with the *Gryphæa dilatata*, that another agrees equally with the young shell of *Trigonia costata*, that a third bears a close resemblance at least to the *Ammonites Harveyii*, while others are identical with the *Ammonites Wallichii*, which is found in the range of the Himalaya. I hope the interest which these species have excited will lead to a more extended investigation of the tract from which they were procured.

Among the illustrations which will accompany a Geological Account of Gurhwal and Sirmou, drawn up by Mr. Royle, is a plate representing certain Shells collected by Mr. Gerard in the elevated valley of the Spiti; these shells may also be identified generically with those of the secondary formations in Europe. Besides these, are given the head and teeth of a small species of Deer, and the tooth of a Rhinoceros, obtained by Messrs. Webb and Trail from the lofty region on the north of the Snowy Mountains; and several teeth of a carnivorous animal, a saurian, and fish, discovered by Mr. Cautley at the southern base of the Himalayan chain.

A list of Fossils found in the Lias of Yorkshire by Mr. Williamson, jun. of Scarborough, will be valuable to us as a local monograph, and still more so as a type to which we may refer the fossils of the same formation in other parts of the country. The author is of opinion that every particular layer is characterized by peculiar organic remains, a statement which must be received with caution; it may be true where the district examined is very small, but published lists, which deserve the greatest confidence, establish beyond all doubt that those species which abound in a formation belong to various beds, and that those species which at one locality are most numerous fail altogether in another.

Mr. Mammatt, one of our Fellows, has embodied the result of forty years' experience in a splendid Work on the Coal-field of Ashby de la Zouch, illustrated with beautiful engravings.

It is a work of considerable labour, and independently of its local interest, contains some remarks on fissures, joints, and "slines," which coming from a practical observer are well entitled to attention. It is to be regretted that the specimens from which the drawings were taken have been too frequently imperfect or indistinct. In other branches of natural history, so intimate are the relations of the several parts, that from the examination of an unknown tooth and a few other bones, the expert physiologist has been enabled in some instances to construct in imagination an unknown animal, the fidelity of which, to nature, subsequent discoveries have established. The laws which determine vegetable forms are more indefinite and obscure; if fragments of plants are to be engraved, they require to be selected with great judgement, and should be confined to those parts of the object which are best defined and most characteristic.

Seen from another point of view, however, these plates become immediately valuable; for though the objects engraved were too indistinct perhaps to enable us to determine the genus, class, or order to which they belong, a correct delineation of them may be sufficient to enable us to identify them with objects found in other coal-fields, perhaps in very distant parts of the world.

The author adopted at an early period the opinion that "Strata are characterized by their Fossils," and he appears to think, that in the coal-field under his consideration, each bed of shale has vegetable impressions of its own. By the precision with which the work is executed, the justice of this opinion is at once put to the test; the successive strata are numbered in regular order, and the names of the plants (where they have names) are attached to the numbers to which they respectively belong. Now, in looking over the list, with a view to the determination of the question before us, I observe *Stigmaria fucoides* at No. 25, 55, 223, 232; *Sigillaria Organum* at No. 37 and 118; *Sigillaria oculata* at No. 74 and 79; *Asterophyllites longifolia* at No. 16, 330, and 370; and *Neuropteris gigantea* at No. 112, 147, 249 a, and 406.

A series of Vegetable Impressions transmitted to us by Mr. De la Beche has given rise to a good deal of discussion. The plants have been examined by Mr. Lindley, and identified at once with those usually found in the Newcastle and other regular coal-fields; they form the roof of certain beds of coal or culm which have long since been observed and worked in the neighbourhood of Bideford in Devonshire, and extend from the shore inland to the distance of about fourteen miles, being about three quarters of a mile in breadth. Along the coast very distinct sections are exposed, both of these beds and their associates. The associated beds have hitherto been generally referred, and with the utmost confidence, to the transition epoch. Many of them appear to me identical with those in the Hartz Mountains, to which the name of grauwacké was in the first instance applied, and which may therefore be considered as the types of that formation. Mr. Smith, indeed, in his geological map of England, refers the Bideford district to the red and dunstone of Brecon and the south-east part of Scotland; whether he applies that term to the old red sandstone exclusively, or to the old red sandstone and grauwacké conjointly, I do not know; at all events, neither he nor any other person has ever expressed a suspicion that the beds under our consideration may be more modern than the limestone of Derbyshire; nor am I aware that such suspicion is entertained even now by any one who has seen them in situ.

Mr. Ainsworth, an active naturalist, who is gone out with Captain Chesney on an expedition to the Euphrates, has published an Account of certain Caves at Ballibunnian on the coast of Kerry. In the bay which bears that name, the cliffs, which rise to the height of a hundred feet, are composed of two beds (varying from thirty to forty feet in thickness) of compact ampelite, divided by seams of the same slate but fissile and anthracitous, and pouring out stream-

lets of water containing iron and salts in solution. Near Hunter's Path are seven beds of anthracite; the laminar and slaty rocks belonging to the great transition clay-slates repose on compact sonorous argillaceous limestone, and considerable beds of quartz occur in the midst of the slate formation; this coast, therefore, seems to be very analogous to that of Bideford: it is desirable to ascertain whether the beds of anthracite are here also accompanied by impressions of plants, and whether they can be identified with those of the independent coal districts.

The distinctness of the Bideford section, and the great experience which Mr. De la Beche possesses in geological surveying, make it highly improbable, I think, that the plants which he has presented to us can belong to any other formation than that to which he has referred them: that the same fossils, vegetable as well as animal, are confined to one particular epoch, and cannot be found in more than one part of the general series, are presumptions, which if countenanced, as to a certain extent they are, by limited experience, more enlarged experience may not unnaturally be expected to overthrow, unless indeed we choose to suppose, amid all the obscurity that surrounds us, that our knowledge has already reached a maximum, and that nothing more can ever be visible than that which we have been accustomed to see; but the case which Mr. De la Beche has stated is not altogether a new case; it does not even contradict our present experience. Coal measures, with their usual plants, have been before found in undoubted grauwacké at the Bocage in Calvados, and you have heard, from one of my predecessors, that they occur in the same relative position at Magdeburg; that they occur in sandstone beds that alternate with mountain limestone in our own country; and that on the southern flank of the Alps they had been discovered by M. Elie de Beaumont in beds of the age of lias.

[To be continued.]

ZOOLOGICAL SOCIETY.

January 27.—Extracts were read from a Letter addressed to the Secretary by J. B. Harvey, Esq., Corr. Memb. Z.S., and dated Teignmouth, January 22, 1835. It was accompanied by a large collection of *Shells* from the south coast of Devonshire, and by specimens of *Echinodermata* and *Crustacea* from the same coast, which the writer presented to the Society. It was also accompanied by drawings of a large specimen of *Caryophyllia Smithii*, now living in Mr. Harvey's possession: the drawings represent the animal shortly after feeding, when it is expanded sufficiently to contain the food, extending rather above the level of the coral and raised in the middle; and also as it appears three or four hours after having been fed, when it expands itself to the fullest extent, and ejects, in the form of *flocculi*, the crude undigested matter.

A Note was read from the Secretary of the United Service Museum, accompanying several skins of *Birds* transmitted for exhibition by direction of the Ornithological Sub-Committee of that

Museum. The specimens were brought under the notice of the Meeting.

The exhibition was resumed of the *Shells* collected by Mr. Cuming on the western coast of South America and among the Islands of the South Pacific Ocean. Those brought before the present Meeting were accompanied by characters by Mr. G. B. Sowerby, and comprised the following species, the characters of which are given in the 'Proceedings.'

Genus Hipponyx. "Of this remarkable genus Mr. Cuming brought home three species in such perfect condition, as respects the shell, as to possess both valves *in situ*. The two specimens which exhibit these three species appear to me so interesting that I shall venture upon a particular description of them. The first, of the species which I have named *Hipp. Mitrula,* is a group of about twenty individuals, of various sizes, from $\frac{1}{30}$ to $\frac{1}{2}$ an inch in diameter, adhering by their lower or flat valves to an irregular piece of stone; the attached valves as usual, are conformed to the irregularities of the surface of the stone, and when they have been at first attached to a cavity, they are hollow: the upper valves are also somewhat modified in form by the same cause, so as to be more or less regular according as the lower valve has adhered to a more or less smooth and even part of the stone. The attached valves have not attained a great degree of thickness, consequently I do not suppose any one of the individuals to be of advanced age; there are, however, several which can only just have occupied their positions on the stone: these are not above $\frac{1}{40}$ part of an inch in diameter, and they show the perfect point of the upper valve, somewhat convoluted and inclined toward the anterior edge. Other individuals, which are placed in a cavity of the stone, are very regular in shape, but have their ridges slightly curved upwards in conformity with the nearly regular vesicular shape of the cavity. The edges of the *lamellæ* near the outer margin in most of the specimens are furnished with a thin fringe of *epidermis,* but the very young shells are destitute of this. An individual of *Hipp. subrufa* is observable among the group of *Hipp. Mitrula*: its *apex* is distinctly spiral and its *epidermis* hairy.

"The second specimen belongs to the species which I have named *Hipp. barbata*. This is a very complete specimen, and reminds me of the beautiful fossil species *Hipp. Cornucopiæ*; it is a small individual, having its attached valve very much thickened and adhering to a much larger one of the same species; its edge is much elevated and it is deeply concave; the free valve is rather smaller, and conical, and its edge is surrounded by the elevated edge of the attached valve."—G. B. S.

Hipp. *Mitrula* (Pileopsis Mitrula, *Lam.* Patella Mitrula, *Auct.*), *subrufa* (Pileopsis subrufus, *Lam.* Patella subrufa, *Dillw.*), *radiata,* Gray, and *barbata*.

Genus Mouretia. Mouret. *Peruviana, stellata,* and *reticulata.*

Genus Siphonaria. Siph. *costata, radiata* (differs from *Siph. costata* rather by its form than by any other character; this being

only a slightly depressed cone, while the last is altogether very flat), *lineolata*, *Pica*, *subrugosa*, *læviuscula*, and *maura*.

Mr. Owen read some Notes (given in the 'Proceedings,') of a Dissection of a *long-tailed Dasyurus*, *Dasyurus macrourus*, Geoff., which recently died at the Society's Gardens. He stated in the course of these notes that the subject was a female, adult, weighing 3lbs. 8½ oz., and measuring from the extremity of the jaws to the root of the tail 1 foot 4 inches, the length of the tail being 1 foot 2½ inches, and that of the head 4 inches. The vaginal orifice and the *anus* were situated within a common outlet, just below the root of the tail. There were six nipples, arranged three on either side, describing three quarters of a circle, and seated within a slight fold of integument, of a corresponding shape, 3 inches anterior to the cloacal outlet.

The external oblique abdominal muscle terminated below in a strong tendon, which was folded inwards, like Poupart's ligament. The abdominal ring consisted of a slit, bounded externally by Poupart's ligament, and internally by the marsupial bone: and Mr. Owen stated it to be his opinion that the marsupial bones are essentially ossifications of the tendons of the external abdominal muscle which constitute the internal or mesial pillars or boundaries of the abdominal rings. The *transversalis abdominis* and internal oblique muscle were distinct.

The *pancreas* was a broad, flattened, branched gland, with a process given off at the splenic end from the main body, so as to produce, in a transverse section, the figure of the letter T. The pancreatic duct joined the biliary just at its termination. The spleen was situated sinistrad and dorsad of the stomach: its weight was 6½ drachms. Its form was compressed, trihedral and T-shaped, as in the *Kangaroo*, but its lesser process was not so long as in that animal. Mr. Owen considers this form as indicative of a relation, hitherto unsuspected, between the *spleen* and the *pancreas*, the small process of the former corresponding to that of the latter.

Mr. Owen also read his Notes on the Anatomy of the *red-backed Pelican* of Dr. Latham, *Pelecanus rufescens*, Gmel., which also are printed in the 'Proceedings' of the Society. The following are extracts from them:

"The *Pelican* which I dissected measured 3 feet 7 inches from the extremity of the beak to the vent, and 10⅓ inches from the extremity of the upper mandible to the nostrils. These are almost concealed slits in the lateral grooves of the upper mandible, just anterior to the skin of the head. They will barely admit the flat end of a probe; and lead almost vertically to the internal apertures of the nasal cavity. The air-cells in the *Pelican*, as in the nearly allied *Bird* the *Gannet*, *Sula Bassana*, Temm., are remarkably extended and diffused over the body: the whole cellular tissue, even to the tips of the wings and the end of the fleshy part of the legs, can be blown up from the *trachea*.

"The extent to which the skeleton of the *Pelican* is permeated by

air has been particularly noted by Mr. Hunter in his celebrated Paper on the air-cells of Birds, in which he throws out a suggestion that it may assist the birds of this species in carrying heavy loads in their large *fauces*. This supposed relation of extended air-cells to a largely developed beak is borne out in the case of the *Hornbill*, in which every bone of the skeleton is permeated by air, but is apparently contradicted by the *Gannet*: I say apparently, because, although the *rami* of the lower jaw do not, in this species, afford suspension to a capacious reservoir as in the *Pelican*, yet the bird may occasionally have to bear away a considerable load, as, for instance, in a large fish seized by its mandibles, and a previous accumulation in its dilatable *œsophagus*.

"Mr. Hunter, it may be remembered, was doubtful on the first publication of his Paper as to the source from which the mandibles derived their gaseous contents: not that he was ignorant of the air-holes in the bones, as he is careful to tell us in the reprint of the Memoir in the 'Animal Œconomy', where he states that the lower jaw of the "*Pelican* is furnished with air, which is supplied by means of the Eustachian tube."

"To ascertain the correctness of this description I sawed across the left *ramus* of the lower jaw; but on blowing into the end of the part attached to the head, I found that the air did not escape as I had expected by the Eustachian tube, (the orifice of which is a slit, situated on the roof of the mouth, one inch behind the posterior or internal *nares*,) but filled, first the air-cells under the throat, and then, passing down the neck, raised the large air-cell above the *furculum*. On dissection I found that the air passed into the lower mandible immediately from an air-cell surrounding the articulation between the jaw and *os quadratum*; which received its air from the lungs by means of the cells passing along the neck and throat, &c. The authority of Mr. Hunter ought not to be set aside by the result of a single experiment; and the possibility of accidental rupture may be urged against the above observation; but it is at all events worthy of being recorded, and should be repeated when opportunity occurs, with the addition of blowing into the Eustachian tube, which I omitted to do.

"There is little to be added to the accounts already given in the works of Cuvier, and of Professor Tiedemann and Carus, of the digestive organs of the *Pelican*. The weak or thin-coated stomach, small *cæca*, and short intestines bespeak its animal diet, and the uniformly capacious *œsophagus*, as well as the superadded faucial bag, may be regarded as pointing to the piscivorous habits of this singular species. It is more difficult to assign the use of the globular cavity interposed between the gizzard and the *duodenum*, which the *Pelican* has in common with some of the piscivorous *Grallæ*, viz. those of the genus *Ardea*. In them the pyloric cavity is very small, but in the *Pelican* it is fully as large in proportion as in the *Crocodiles*, which alone possess it among *Reptiles*. In the *Pelican* here described the pyloric cavity measured 1½ inch in diameter, communicated by a small transverse aperture with the gizzard, and by an opposite one,

of smaller size and obliquely placed, with the *duodenum*. Its lining membrane is villous and vascular, and was in this instance tinged with bile, which must have entered by regurgitation, as none of the biliary ducts enter here.

"As the *Pelican* belongs to that group of *Natatores*, the *Totipalmes* of Cuvier, which contains species approximating most closely to the *Raptorial Birds*, and which are almost the only *Birds* of this order, as Cuvier observes, (Règne An., nouv. ed., i. p. 561,) that perch, I did not fail to try the common experiment suggested by Borelli's observations on the effect which bending the leg- and ancle-joints might have upon the toes: the latter, however, exhibited no corresponding inflection. In perfect agreement with this is the observation that the *Pelicans* do not perch when they go to rest."

XVIII. *Intelligence and Miscellaneous Articles.*

COMPOSITION OF IODIDE OF IRON.

To the Editors of the Philosophical Magazine and Journal of Science.

GENTLEMEN,

DR. THOMSON having stated in the first volume of his Inorganic Chemistry, that the composition of iodide of iron had not been experimentally determined, I boiled 126 grains of iodine with iron filings in distilled water; the solution was of a light green colour: the residue of the filings having been washed, the mixed solutions were evaporated in a flask, and by deducting the weight of it, 190·5 grains of solid iodide of iron were obtained. This was dissolved in water, and filtered to separate a little oxide of iron left by the dissipation of part of the iodine during evaporation: its weight was 1·2 grain. The solution divided into two equal parts: one, precipitated by nitrate of silver, afforded 113·4 grains of iodide, of silver; the other, boiled with nitric acid to peroxidize the iron, was precipitated by ammonia, and gave 19 grains of peroxide of iron. Now, 113·4 grains of iodide of silver indicate 61·06 grains of iodine, and 19 grains peroxide of iron, 13·3 of iron; these doubled give 122·12 and 26·6 respectively as the combining proportions of iodine and iron: from these numbers we may conclude, that iodide of iron is composed of an equivalent of each of its elements. The 1·2 grain of oxide of iron deposited indicates by the above numbers 3·8 grains of iodine dissipated, there being a loss of $\frac{1}{10}$ grain of iodine and $\frac{6}{10}$ grain of iron in the whole. 148·77 grains of iodide of iron and 1·2 grain of oxide, subtracted from 190·5 grains, the weight of the solid product obtained, give 40·53 for water; this not indicating satisfactorily the combining proportion of water, 63 grains of iodine were boiled with excess of iron, filtered, and carefully evaporated, finishing the evaporation in a water-bath: 99 grains of iodide of iron were obtained, a dark-greenish brown and crystalline mass, apparently in rhombic crystals, but not distinct enough to determine clearly the form. According to the previous experiment 77 grains of the 99 would be anhydrous

iodide of iron, the remainder being water; this corresponds very nearly with 5 equivalents of water to 1 equivalent of anhydrous iodide of iron; the deficiency in the experiment of ½ grain may, I conceive, be traced to the dissipation of iodine, a very small quantity of oxide of iron having been deposited during evaporation, as in the previous experiment.

Thus iodide of iron is composed of

1	equivalent of	iodine	126
1	do.	iron	28
5	do.	water..........	45
			199

I have not been able to prepare anhydrous iodide of iron; for when the heat is continued long enough to dissipate the water, iodine is expelled with it, the residuum being peroxide of iron.

I am, Gentlemen, yours respectfully,

St. Thomas's Hospital, July 20, 1835. J. D. Smith.

PARALLEL, BY NEWTON, UPON THE CORPUSCULAR THEORY, TO MR. TALBOT'S EXPLANATION OF CERTAIN PHÆNOMENA OF INCANDESCENCE UPON THE UNDULATORY THEORY.

The following was intended to appear as a note to Mr. Talbot's paper, p. 115, but was not prepared in time for insertion in its proper place.

[Sir Humphry Davy, in his Elements of Chemical Philosophy, notices, in the following terms, Newton's suggestions towards the explanation, upon the corpuscular hypothesis, of certain phænomena nearly related to those above explained by Mr. Talbot upon the undulatory theory, of whose views they afford a curious parallel; but in which, we conceive, the superiority of the undulatory theory is very manifest.—" Newton," Sir Humphry Davy says, " has put the query whether light and common matter are not convertible into each other; and adopting the idea that the phænomena of sensible heat depend upon vibrations of the particles of bodies, supposes that a certain intensity of vibrations may send off particles into free space, and that particles in rapid motion, in right lines, in losing their own motion, may communicate a vibratory motion to the particles of terrestrial bodies. (Elem. of Chem. Philos., p. 215—216. —Edit.]

SCIENTIFIC ASSOCIATION OF GERMANY: MEETING AT BONN FROM SEPTEMBER 17TH TO 27TH.

The annual meeting of this body is to be held this year at Bonn on the Rhine, from the 17th to the 27th of September. At the meeting last year at Stuttgart, Dr. Christian Friederich Harless, Privy Councillor of Prussia, and Professor of Medicine in the University of Bonn, and Dr. Jacob Nöggerath, one of the Directors in Chief of the Council of Mines for the Rhenish Provinces of Prussia, were respectively chosen President and Secretary of the ensuing meeting.

The Committee appointed to superintend the preliminary arrangements have already received notice of the intended presence of several of the most eminent men of science in Europe. The Geological Society of France meets in the beginning of September at Mezieres, and after examining the country there, and around Namur, Liege, and Aix la-Chapelle, joins the German Association at Bonn. The attractions of such an assemblage will be greatly heightened by the beauty of the country around the place of meeting; and the neighbouring Siebengebirge, Laacher See, and Eifel will present especial objects of interest to the geologist.

There will be sufficient time to go to Bonn after the meeting of the British Association in Dublin, and we hope that our own country will be worthily represented in all the departments of physical and medical science. Those who mean to go will do well to give notice in due time; in order that they may not be disappointed as to accommodations. We know that in the true spirit of German hospitality, the Committee are anxious to provide comfortable quarters for all strangers; but the town is small, and therefore they should have as early notice as possible. Letters should be addressed to Professor Nöggerath.

ON PHOSPHURET OF AZOTE. BY M. LIEBIG.

This compound is obtained by saturating solid chloride of phosphorus with dry ammoniacal gas: a white mass is formed, from which cold water separates a great quantity of muriate of ammonia. From this it appears that there is reaction and decomposition when these substances are mixed, and what especially proves it is, that on cooling during the preparation, the quantity of muriate of ammonia dissolved does not diminish. The water used for this solution does not contain a trace of phosphate, so that it may be concluded that all the phosphorus remains in the residue.

The washing may be continued for several weeks, and the last waters always give a precipitate with nitrate of silver; so that the insoluble white substance appears to be a compound of phosphuret of azote and muriate of ammonia, and which is indefinitely decomposed by washing.

The muriate of ammonia is more readily dissolved when the substance is repeatedly boiled with a solution of potash, and afterwards with dilute nitric or sulphuric acid; when it is long boiled with potash, ammonia is continually evolved, without the substance undergoing any change of appearance. A portion of it thus treated was well washed and dried with much care: 0·738 gramme of it were acted on by a mixture of carbonate of soda and a little nitre. The matter treated with water was saturated with nitric acid, and nitrate of silver added to it, gave 0·015 of chloride = 0·0037 of chlorine, or 0·0005 per cent. This small quantity of chlorine which the substance still retained, proves how difficult it is to remove the last traces of muriate of ammonia.

The white substance, purified by long ebullition with potash, is not pure phosphuret of azote, for it contains a quantity of com-

bined water; this water appears certainly to have taken the place of the muriate of ammonia, with which it was previously in combination, for when the substance is heated after having been completely dried it disengages a large quantity of ammoniacal gas, without undergoing any change of appearance. If it be heated with oxide of copper a considerable quantity of water is obtained, amounting to 24·27 per cent.

100 parts gave 92·47 of phosphoric acid, equivalent to 40·68 of phosphorus. The determination could not be exactly made; too little was always obtained, on account of the formation of nitrous acid. 0·193 gramme gave a quantity of azotic gas which indicated 28·526 per cent.; but estimating by the deficiency of weight required to make 100 with the phosphorus and water, it should be rather more than 35 per cent.

M. Liebig concludes its composition to be nearly

1 atom phosphorus	196·155	40·4
2 atoms azote	177·436	36·5
1 atom water............	112·479	23·1
	485·670	100·0

If the vapour of heated muriate of ammonia be passed over solid chloride of phosphorus, the muriate is decomposed, and a solid shining white substance remains, which resists the action of heat, and which is phosphuret of azote nearly pure; its chemical properties are identical with those procured by means of the liquid chloride already mentioned, but it always contains from about 1·5 to about 3 per cent. of chlorine, from which it is difficult to separate it.—*Ann. de Chim. et de Phys.*, tom. lvii. p. 426.

CRYSTALLINE FORM OF KUPFERBLÜTHE.

Professor Luckow of Jena has ascertained that the kupferblüthe of the German mineralogists, "capillary red oxide of copper," crystallizes in six-sided prisms, which afford cleavages parallel to the faces of a rhombohedron of 99° 15′. This mineral is chemically identical with the octahedral oxide of copper, and therefore affords a new instance of *dimorphism.*

TEMPERATURE OF VAPOURS FROM BOILING SOLUTIONS.

Professor Rudberg has ascertained, by a series of careful experiments, that the temperature of the vapour arising from a boiling solution of any salt is independent of the nature and quantity of the salt, and is absolutely the same as that of the vapour of pure water under the same atmospheric pressure.

This is in direct opposition to the statements of Biot, Gay-Lussac, and Pouillet, who assert that the temperature of the vapour arising from a boiling saline solution is the same as the temperature of the highest stratum of the liquid.

The translator of the preceding notice was unable to detect any difference between the temperature of the vapour of water and that of a solution of common salt which boiled at 221° Fahr.

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick near London, and by Mr. VEALL, at Boston.

Days of Month. 1835.	Barometer. London. Max.	Barometer. London. Min.	Barometer. Boston. 8½ A.M.	Thermometer. London. Max.	Thermometer. London. Min.	Thermometer. Bost. 8½ A.M.	Wind. Lond.	Wind. Bost.	Rain. Lond.	Rain. Bost.
June 1	30·050	29·914	29·48	63	42	58	S.	calm	0·02	...
2	30·030	29·990	29·60	71	50	62	S.	E.	·17	...
3	29·926	29·899	29·44	74	51	58	S.	calm	...	·13
☽ 4	30·051	29·984	29·54	64	48	60	NE.	calm	...	·11
5	30·105	30·087	29·65	63	51	55·5	NE.	N.	...	...
6	30·100	30·074	29·60	81	52	65	NE.	E.	...	...
7	30·139	30·097	29·63	82	53	65	SE.	calm	...	...
8	30·154	30·081	29·52	85	52	72	S.	calm	...	...
9	30·261	30·195	29·54	85	50	73	W.	calm	...	...
○ 10	30·326	30·280	29·60	87	62	75·5	SE.	calm	...	...
11	30·359	30·330	29·63	85	63	76	SE.	calm	...	...
12	30·375	30·349	29·73	8	62	61·5	NE.	calm	...	·39
13	30·337	30·278	29·77	71	47	57·	N.	calm	...	...
14	30·242	30·196	29·72	75	51	65	N.	calm	...	...
15	30·234	30·206	29·60	79	52	67	N.	W.	...	...
16	30·264	30·152	29·62	78	60	68	N.	W.	...	...
17	30·121	30·061	29·46	77	53	67	N.	NW.	0·1	...
☾ 18	30·139	30·089	29·50	78	45	58	N.	calm	...	·05
19	30·166	30·031	29·62	67	53	59	W.	NW.	...	...
20	30·001	29·960	29·38	68	54	62·5	W.	NW.	...	...
21	30·046	29·937	29·50	75	62	62	W.	NW.	...	...
22	29·843	29·705	29·20	75	50	65	SW.	NW.	·30	...
23	29·765	29·658	29·08	65	42	58	SW.	NW.	·02	·16
24	29·567	29·253	29·04	62	44	56	SW.	NW.	·26	·05
25	29·500	29·426	28·86	54	37	51	NW.	NW.	·20	·96
● 26	29·893	29·536	29·31	60	43	55	SW.	NW.	1·00	·13
27	30·236	29·989	29·38	61	41	52	W.	N.	·01	·06
28	30·259	30·172	29·68	69	40	56	NW.	NE.	...	...
29	30·239	30·172	29·72	71	41	56	N.	NE.	...	...
30	30·144	30·112	29·55	76	45	59	S.	calm	...	...
	30·375	29·253	29·49	87	37	61·8			1·99	2·04

Remarks.

London.— June 1. Rain: cloudy and fine. 2. Very fine: rain at night. 3. Sultry. 4. Fine. 5. Hazy. 6. Fine. 7. Sultry. 8—12. Hot. 13—16. Very fine. 17. Cloudy: lightning at night. 18, 19. Fine. 20. Cloudy. 21, 22. Fine. 23. Cloudy and fine: rain at night. 24. Cold rain. 25. Cold and windy: heavy rain: cold at night. 26. Clear: heavy rain. 27. Fine, but cold. 28. Fine. 29. Cold dew: fine. 30. Fine.—The first part of the month was generally very warm, but with very little rain, a supply of which was much wanted before it commenced on the 22nd. On the 26th, with the change of the moon, rain fell to the depth of an inch. During the last week of the month the nights were unseasonably cold.

Boston.—June 1. Cloudy. 2. Fine: rain P.M. 3. Cloudy: rain A.M. and P.M. 4, 5. Cloudy. 6—9. Fine. 10. Fine: 3 P.M. thermometer 82°. 11. Fine: 1 P.M. thermometer 85°: thunder, lightning, and rain P.M. 12, 13. Cloudy. 14—16. Fine. 17. Cloudy. 18. Cloudy: rain early A.M. 19. Fine. 20. Cloudy. 21. Fine. 22. Cloudy. 23. Fine: rain early A.M.; rain P.M. 24. Fine: stormy with rain A.M. and P.M. 25. Cloudy and stormy: rain A.M. and P.M. 26. Fine: rain P.M. 27—30. Fine.

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[THIRD SERIES.]

SEPTEMBER 1835.

XIX. *On the Double Achromatic Object Glass.* *By* J. W. Lubbock, *Esq., V.P. & Treas. R.S.**

IN the Philosophical Transactions for 1821, Sir John F. W. Herschel has entered minutely into the theory of the double achromatic object glass, and has given various useful tables showing the curvatures of the lenses to be employed in order that the spherical and chromatic aberrations may be destroyed†. Sir John Herschel seems to have intended to consider at some future time the modifications which would result in the inquiry when the thicknesses are not neglected, but I am not aware that he ever carried this intention into effect. I have therefore endeavoured to put the expressions which occur in the theory of spherical aberration in the most advantageous form. In this form they admit of extension to the case when the thicknesses of the lenses and the interval between them are considered, without any difficulty.

Whether or not opticians will ever trust in great measure to theory for the curvatures of the lenses, taking care previously to ascertain with precision the optical characters of the glass of which they are composed, may admit of doubt; but it would certainly be useful to know the figures of the lenses and other circumstances connected with any large object glass whose performance is considered perfect, in order

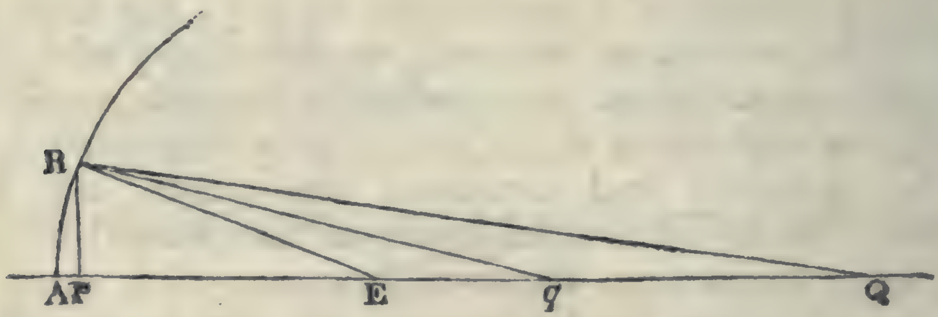
* Communicated by the Author.

† A notice of Sir John Herschel's paper will be found in Phil. Mag., vol. lx. p. 147.—Edit.

that these data might be compared with the results given by theory *. Unfortunately no such measures have yet been published, as far as I am aware, except those in Sir David Brewster's Encyclopædia, under the article *Achromatic Telescopes*; but certain data are there wanting to render them complete. I am informed that the universal practice in this country and in France has been to make the flint lens double concave, and not concavo-convex as theory indicates, which circumstance seems to me very extraordinary. I am told that Fraunhofer employed the latter construction; but it would seem from the representation of an object glass incidentally given in a plate which accompanies his paper on the lines in the spectrum, that he did not do so invariably.

Let EA or $ER = r$ $\quad AQ = \Delta \quad Aq = \Delta' \quad RP = y$

QR is the incident ray meeting the axis AE in Q, Rq the refracted ray, the direction of which cuts the axis in q, E the centre of the surface of which RA is a section. RP is drawn perpendicular to AE. The notation employed is that of Mr. Coddington's Elementary Treatise on Optics, 2nd edit., p. 54.



$$m = \frac{\sin ERQ \sin REq}{\sin REQ \sin ERq} = \frac{EQ \cdot Rq}{RQ \cdot Eq} = \frac{(\Delta - r)\sqrt{\{(\Delta' - AP)^2 + y^2\}}}{(\Delta' - r)\sqrt{\{(\Delta - AP)^2 + y^2\}}}$$

m being the *index of refraction* for the substance of which the lens is formed, which is bounded by the sphere of which E is the centre.

$$AP = \frac{y^2}{2r - AP} \qquad AP = \frac{y^2}{2r}\left(1 + \frac{y^2}{4r^2}\right) \text{ nearly}$$

$$(\Delta' - AP)^2 = \Delta'^2 - \frac{\Delta' y}{r}\left(1 + \frac{y^2}{4r^2}\right) + \frac{y^4}{4r^2} + y^2$$

* The curvatures of the surfaces can be ascertained very correctly by the *Spherometer*, an instrument described by M. Biot. (*Traité de Physique*, vol. iv. p. 343.)

$$= \Delta'^2 \left\{1 - \frac{1}{\Delta'}\left(\frac{1}{r} - \frac{1}{\Delta'}\right)\left(y^2 + \frac{y^4}{4r^2}\right)\right\}$$

$$m^2 (\Delta' - r)^2 \Delta^2 \left\{1 - \frac{1}{\Delta}\left(\frac{1}{r} - \frac{1}{\Delta}\right)\left(y^2 + \frac{y^4}{4r^2}\right)\right\}$$

$$= (\Delta - r)^2 \Delta'^2 \left\{1 - \frac{1}{\Delta'}\left(\frac{1}{r} - \frac{1}{\Delta'}\right)\left(y^2 + \frac{y^4}{4r^2}\right\}\right)$$

$$\frac{1}{r} - \frac{1}{\Delta'} = \frac{1}{m}\left\{\frac{1}{r} - \frac{1}{\Delta}\right\} + \left(\frac{1}{r} - \frac{1}{\Delta}\right)\left(\frac{1}{r} - \frac{1}{\Delta}\right)\left(\frac{1}{\Delta} - \frac{1}{m\Delta'}\right)\frac{y^2}{2}$$

$$+ \left(\frac{1}{r} - \frac{1}{\Delta'}\right)\left(\frac{1}{r} - \frac{1}{\Delta}\right)\left\{\frac{1}{\Delta}\left(\frac{1}{r^2} + \frac{1}{\Delta}\left(\frac{1}{r} - \frac{1}{\Delta}\right)\right.\right.$$

$$\left.\left. - \frac{1}{m'\Delta}\left(\frac{1}{r^2} + \frac{1}{\Delta}\left(\frac{1}{r} - \frac{1}{\Delta'}\right)\right)\right\}\frac{y^4}{8} +, \&c.$$

Neglecting y^2,

$$\frac{1}{r} - \frac{1}{\Delta} = \frac{m}{m-1}\left(\frac{1}{\Delta'} - \frac{1}{\Delta}\right) \qquad \frac{1}{r} - \frac{1}{\Delta'} = \frac{1}{m-1}\left(\frac{1}{\Delta'} - \frac{1}{\Delta}\right).$$

Hence, neglecting y^4,

$$\frac{1}{\Delta'} = \frac{m-1}{mr} + \frac{1}{m\Delta} + \left\{\frac{1}{m\Delta'} - \frac{1}{\Delta}\right\}\left\{\frac{1}{\Delta'} - \frac{1}{\Delta}\right\}^2 \frac{my^2}{2(m-1)^2}.$$

Hence, if r_1, r_2, r_3, r_4 are the radii of four spherical surfaces; $\Delta_1, \Delta_2, \Delta_3, \Delta_4$, the distances from the vertex at which the ray intersects their common axis after refraction at the first, second, third, and fourth surfaces respectively, neglecting the *thicknesses*; m, the *index of refraction* for the substance of which the lens is formed which is bounded by the first and second surfaces; m', the *index of refraction* for the substance of which the lens is formed which is bounded by the third and fourth surfaces; then, neglecting y^4,

$$\frac{1}{\Delta} = \frac{m-1}{mr} + \frac{1}{m\Delta} + \left\{\frac{1}{m\Delta_1} - \frac{1}{\Delta}\right\}\left\{\frac{1}{\Delta_1} - \frac{1}{\Delta}\right\}^2 \frac{my^2}{2(m-1)^2}$$

$$\frac{1}{\Delta_2} = -\frac{m-1}{r_2} + \frac{m}{\Delta_1} + \left\{\frac{1}{\Delta_2} - \frac{1}{m\Delta_1}\right\}\left\{\frac{1}{\Delta_2} - \frac{1}{\Delta_1}\right\}^2 \frac{m^2y^2}{2(m-1)^2}$$

$$\frac{1}{\Delta_3} = \frac{m'-1}{r_3} + \frac{1}{m'\Delta_2} + \left\{\frac{1}{m'\Delta_3} - \frac{1}{\Delta_2}\right\}\left\{\frac{1}{\Delta_3} - \frac{1}{\Delta_2}\right\}^2 \frac{m'y^2}{2(m'-1)^2}$$

$$\frac{1}{\Delta_4} = -\frac{m'-1}{r_4} + \frac{m'}{\Delta_3} + \left\{\frac{1}{\Delta_4} - \frac{1}{m'\Delta_3}\right\}\left\{\frac{1}{\Delta_4} - \frac{1}{\Delta_3}\right\}^2 \frac{m'^2y^2}{2(m'-1)^2}$$

$$\frac{1}{\Delta_4} = -(m'-1)\left\{\frac{1}{r_4} - \frac{1}{r_3}\right\} - (m-1)\left\{\frac{1}{r_2} - \frac{1}{r_1}\right\} + \frac{1}{\Delta}$$

$$+\left\{\left\{\frac{1}{m\Delta_1}-\frac{1}{\Delta}\right\}\left\{\frac{1}{\Delta_1}-\frac{1}{\Delta}\right\}^2+\left\{\frac{1}{\Delta_2}-\frac{1}{m\Delta_1}\right\}\left\{\frac{1}{\Delta^2}-\frac{1}{\Delta_1}\right\}^2\right]\frac{m^2 y^2}{2(m-1)^2}$$

$$+\left\{\left\{\frac{1}{m'\Delta_3}-\frac{1}{\Delta_2}\right\}\left\{\frac{1}{\Delta_3}-\frac{1}{\Delta_2}\right\}^2+\left\{\frac{1}{\Delta_4}-\frac{1}{m'\Delta_3}\right\}\left\{\frac{1}{\Delta_4}-\frac{1}{\Delta_3}\right\}^2\right\}\frac{m'^2 y^2}{2(m'-1)^2}$$

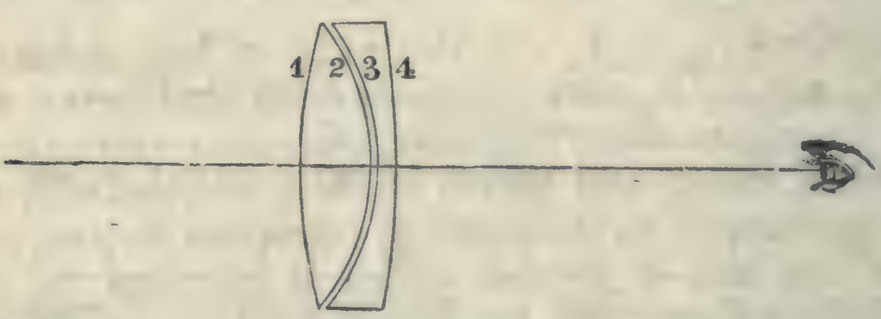
This expression for $\frac{1}{\Delta_4}$ may be put into the following more convenient form :

$$\frac{1}{\Delta_4}=-\left(m'-1\right)\left\{\frac{1}{r_4}-\frac{1}{r_3}\right\}-\left(m-1\right)\left\{\frac{1}{r_2}-\frac{1}{r_1}\right\}+\frac{1}{\Delta}$$

$$+\left(\frac{1}{\Delta_2}-\frac{1}{\Delta}\right)\left\{\left(\frac{1}{\Delta_2}-\frac{1}{\Delta_1}\right)^2+\left(\frac{1}{\Delta}-\frac{1}{m\Delta_1}\right)\left(\frac{1}{\Delta}-\frac{2}{\Delta_1}+\frac{1}{\Delta}\right)\right\}\frac{m^2 y^2}{2(m-1)^2}$$

$$+\left(\frac{1}{\Delta_4}-\frac{1}{\Delta_2}\right)\left\{\left(\frac{1}{\Delta_4}-\frac{1}{\Delta_3}\right)^2+\left(\frac{1}{\Delta_2}-\frac{1}{m'\Delta_3}\right)\left(\frac{1}{\Delta_4}-\frac{2}{\Delta_3}+\frac{1}{\Delta_2}\right)\right\}\frac{m'^2 y^2}{2(m'-1)}.$$

In order to get rid of spherical *aberration*, the quantities Δ_1, Δ_2, Δ_3, Δ_4 must be determined by the condition that the coefficient of y^2 in the last equation = zero. In the construction recommended by Sir John Herschel, the object glass consists of a double-convex lens of crown glass and a concavo-convex lens of flint glass. This combination is represented in the figure underneath, where the four surfaces are numbered



in the order in which the light traverses them in its passage from the object to the eye.

Neglecting y^2, if

$$\frac{1}{\Delta_1}=f_1+\frac{1}{m\,\Delta} \qquad \frac{1}{\Delta_2}=f_2+\frac{1}{\Delta}$$

$$\frac{1}{\Delta_3}=f_3+\frac{1}{m'\,\Delta} \qquad \frac{1}{\Delta_4}=f_4+\frac{1}{\Delta},$$

and if ϖ be the ratio of the *dispersive power* of the crown glass to the *dispersive power* of the flint, it is easy to show that the condition of achromaticity requires

$$\frac{1}{\Delta_4}-\frac{1}{\Delta_2}=-\varpi\left\{\frac{1}{\Delta_2}-\frac{1}{\Delta}\right\};$$

or, for parallel rays, that is, when Δ is infinite and $\frac{1}{\Delta} = 0$,

$$\frac{1}{\Delta_4} - \frac{1}{\Delta} = - \frac{\varpi}{\Delta_2} \qquad f_2 = \frac{f_4}{1-\varpi}$$

f_4 may be assumed at pleasure, and f_2 being obtained from the last equation, f_2 and f_4 may be considered as given. It remains to determine f_1, f_3, in doing which another arbitrary condition is admissible.

If $\varpi \frac{m'^2 (m-1)^2}{(m'^2-1)^2 m^2} = \lambda$, for parallel rays,

$$(f_2-f_1)^2 - \frac{f_1}{m} (f_2 - 2f_1)$$

$$= \lambda \left\{ \{f_4-f_3\}^2 + \{f_2 - \frac{f_3}{m}\} \ \{f_4 - 2f_3 + f_2\} \right\}.$$

The last equation coincides with the equation (z) of Sir John Herschel in the Phil. Trans. for 1821, p. 258.

Since

$$\frac{1}{\Delta_2} = - \frac{m-1}{r_2} + \frac{m}{\Delta_1} \qquad \frac{1}{r_2} = \frac{m}{m-1} \left\{ \frac{1}{\Delta_1} - \frac{1}{m \Delta_2} \right\}.$$

$$\frac{1}{\Delta_3} = \frac{m'-1}{m' r_3} + \frac{1}{m' \Delta_2} \qquad \frac{1}{r_3} = \frac{m'}{m'-1} \left\{ \frac{1}{\Delta_3} - \frac{1}{m' \Delta_2} \right\}$$

if the radii of the second and third surfaces are equal

$$\frac{m}{m-1} \left\{ \frac{1}{\Delta_1} - \frac{1}{m \Delta_2} \right\} = \frac{m'}{m'-1} \left\{ \frac{1}{\Delta_3} - \frac{1}{m' \Delta_2} \right\}$$

$$\frac{m}{m-1} \left\{ f_1 - \frac{f_2}{m} \right\} = \frac{m'}{m'-1} \left\{ f_3 - \frac{f_2}{m'} \right\}.$$

This condition was originally suggested by Clairaut; but according to Sir John Herschel, "when the average values of the indices of refraction, such as are likely to occur most frequently, are employed, the construction becomes imaginary for the more dispersive kinds of glass; and within the limits for which it is real, the radii change so rapidly as to render it difficult to interpolate between their calculated values."

Hence, instead of this condition, Sir John Herschel recommends another, which results from making equal to zero the coefficient of $\frac{1}{\Delta}$ in the quantity

$$\left\{\frac{1}{\Delta_2} - \frac{1}{\Delta_1}\right\}^2 + \left\{\frac{1}{\Delta_2} - \frac{1}{m\Delta_1}\right\}\left\{\frac{1}{\Delta_2} - \frac{2}{\Delta_1} + \frac{1}{\Delta}\right\}^2$$

$$= \lambda\left\{\frac{1}{\Delta_4} - \frac{1}{\Delta_3}\right\}^2 + \left\{\frac{1}{\Delta_2} - \frac{1}{m'\Delta_3}\right\}\left\{\frac{1}{\Delta_4} - \frac{2}{\Delta_3} + \frac{1}{\Delta_2}\right\}^2,$$

in order that the telescope may be adapted for seeing nearer as well as more remote objects. This condition gives

$$\frac{2(m-1)}{m}\{f_2 - f_1\} + \frac{(m-1)(m+1)}{m^2}\{f_2 - 2f_1\} - \frac{2(m-1)}{m} f_1$$

$$= \lambda\left\{\frac{2(m'-1)}{m'}\{f_4 - f_3\} + \frac{(m'-1)(m'+1)}{m'^2}\{f_4 - 2f_3 + f_2\}\right.$$

$$\left. + \frac{2(m'-1)}{m'}\left\{f_2 - \frac{1}{m'} f_3\right\}\right\}.$$

The last equation coincides with the equation (A) of Sir John Herschel, p. 258. This equation is to be combined with the equation, p. 165, line 8, which may be put into the form

$$f_1^2 - f_2\left(\frac{2m+1}{m+2}\right)f_1 + \frac{m}{m+2}f_2^2$$

$$= \lambda\frac{m(m'+2)}{(m+2)m'}\left\{f_3^2 - (f_4 + f_2)\left(\frac{2m'+1}{m'+2}\right)f_3\right.$$

$$\left. + \frac{m'}{m'+2}\left\{f_4^2 + f_2(f_4 + f_2)\right\}\right\}.$$

Having found f_1, f_2, f_3, f_4, the radii of curvature may be obtained by means of the equations

$$f_1 = \frac{m-1}{m r_1} \qquad f_2 = -\frac{m-1}{r_2} + m f_1$$

$$f_3 = \frac{m'-1}{m' r_3} + \frac{f_2}{m'} \qquad f_4 = -\frac{(m'-1)}{r_4} + m' f_3.$$

If $m = 1{\cdot}524$, $m' = 1{\cdot}585$, $\varpi = {\cdot}60$, (which are the average values,) Sir John Herschel finds, assuming $f_4 = -{\cdot}1$, and with the conditions which he recommends,

$$r_1 = -6{\cdot}7069 \qquad r_2 = 3{\cdot}0488 \qquad r_3 = 3{\cdot}0640 \qquad r_4 = 14{\cdot}2937.$$

With these curvatures

$$f_1 = -{\cdot}051265 \qquad f_2 = -{\cdot}25 \qquad f_3 = -{\cdot}03727 \qquad f_4 = -{\cdot}1$$

$$f_2 - f_1 = -{\cdot}19873 \qquad -\frac{f_1}{m} = .03364 \qquad f_2 - 2f_1 = -{\cdot}14746$$

$f_4 - f_3 = -{\cdot}06272 \qquad f_2 - \frac{f_3}{m'} = -{\cdot}22648 \qquad f_4 - 2f_3 + f_2 = -{\cdot}27544$

$-\frac{f_1{}^3}{m} = -{\cdot}0000884 \qquad \left(f_2 - \frac{f_1}{m}\right)(f_2 - f_1)^2 = -{\cdot}0085448$

$\left(\frac{f_3}{m} - f_2\right)(f_3 - f_2)^2 = {\cdot}010249 \qquad \left(f_4 - \frac{f_3}{m'}\right)(f_4 - f_3)^2 = -{\cdot}000301.$

The preceding values will be found to destroy the coefficient of y^2 in the expression for f_4. The values of the radii are taken from a very useful table given by Sir John Herschel in the paper before alluded to.

One advantage which seems to me to result from the form into which the equations have been put in this short view of the theory of aberration is the facility with which the thicknesses of the lenses and the interval between them can be taken into account. For if Δ_1, Δ_2, Δ_3, Δ_4, are measured, as before, from the points at which the surfaces cut the axis respectively, we have obviously

$$\frac{1}{\Delta_1} = \frac{m-1}{m r_1} + \frac{1}{m\Delta} + \left\{\frac{1}{m\Delta_1} - \frac{1}{\Delta}\right\}\left\{\frac{1}{\Delta_1} - \frac{1}{\Delta}\right\}^2 \frac{m y^2}{2(m-1)^2},$$

or, for parallel rays,

$$\frac{1}{\Delta_1} = \frac{m-1}{m r_1} + \frac{y^2}{2\Delta_1{}^3(m-1)^2};$$

and if t_1 be the thickness of the anterior lens, or the interval between the intersections of the axis by the first and second surfaces,

$$\frac{1}{\Delta_2} = -\frac{m-1}{r_2} + \frac{m}{\Delta_1 + t_1}$$
$$+ \left\{\frac{1}{\Delta_2} - \frac{1}{m(\Delta_1 + t_1)}\right\}\left\{\frac{1}{\Delta_2} - \frac{1}{\Delta_1 + t_1}\right\}\frac{(\Delta_1 + t_1)^2 m^2 y^2}{2\Delta_1{}^2(m-1)^2}.$$

In Sir John Herschel's construction the interval between the lenses may be made inconsiderable. In this case

$$\frac{1}{\Delta_3} = \frac{m'-1}{m' r_3} + \frac{1}{m'\Delta_3}$$
$$+ \left\{\frac{1}{m'\Delta_2} - \frac{1}{\Delta_2}\right\}\left\{\frac{1}{\Delta_3} - \frac{1}{\Delta_2}\right\}^2 \frac{m'(\Delta_1 + t_1)^2 y^2}{2\Delta_1{}^2(m'-1^2},$$

and if t_2 be the thickness of the second lens,

$$\frac{1}{\Delta_4} = -\frac{m'-1}{r_4} + \frac{m}{\Delta_3 + t_2}$$
$$+ \left\{\frac{1}{\Delta_4} - \frac{1}{m'(\Delta_3 + t_2)}\right\}\left\{\frac{1}{\Delta_4} - \frac{1}{\Delta_3 + t_2}\right\}^2 \frac{m'^2(\Delta_1 + t_1)^2(\Delta_3 + t_2)^2 y^2}{2\Delta_1{}^2\Delta_3{}^2(m'-1)^2}.$$

In this construction the thickness of the second lens may also be made inconsiderable, and as $\frac{1}{\Delta_4} - \frac{1}{m' \Delta_3}$ and $\frac{1}{\Delta_4} - \frac{1}{\Delta_3}$ are small quantities, (see the numerical values, p. 167,) t_2 may certainly be disregarded in the coefficient of y^2 in the expression for $\frac{1}{\Delta_4}$.

Neglecting y^2 and $\frac{1}{\Delta}$,

$$f_2 = \frac{1}{\Delta_2} = -(m-1)\left\{\frac{1}{r_2} - \frac{1}{r_1}\right\} - \frac{m t_1}{\Delta_1^2}$$

$$f_4 = \frac{1}{\Delta_4} = -(m'-1)\left\{\frac{1}{r_4} - \frac{1}{r_3}\right\} - (m-1)\left\{\frac{1}{r_2} - \frac{1}{r_1}\right\}$$
$$- \frac{m' t_2}{\Delta_3^2} - \frac{m t_1}{\Delta_1^2}$$

$$\delta m' \left\{\frac{1}{r_4} - \frac{1}{r_3} + \frac{t_2}{\Delta_3^2}\right\} + \delta m \left\{\frac{1}{r_2} - \frac{1}{r_1} + \frac{t_1}{\Delta_1^2}\right\} = 0$$

$$f_4 - f_2 + \frac{t_2}{\Delta_3^2} = - \frac{\frac{\delta m}{m-1}}{\frac{\delta m'}{m'-1}} \left\{f_2 + \frac{t_1}{\Delta_1^2}\right\}$$

$$= - \varpi \left\{f_2 + \frac{t_1}{\Delta_1^2}\right\}$$

$$f_4 = (1-\varpi) f_2 - \frac{\varpi t_1}{\Delta_1^2}.$$

In the numerical example given, p. 166, supposing the aperture $= 1$, and the thickness of the double-convex lens to vanish at the circumference of the field, $t_1 = \cdot 06$ nearly.

$$f_3^2 - \frac{(2m'+1)}{(m'+2)}(f_4 + f_2) f_3 + \frac{m'}{m'+2}\left\{f_4^2 + f_2(f_4 + f_2)\right\}$$
$$+ \frac{m^2 (m'-1)^2}{(f_4 - f_2)(m'+2) m'(m-1)^3}\left\{\left(\frac{1}{\Delta_2} - \frac{1}{m(\Delta_1 + t_1)}\right)\right.$$
$$\left.\left(\frac{1}{\Delta_2} - \frac{1}{\Delta_1 + t_1}\right)^2 + \frac{1}{m \Delta_1 (\Delta_1 + t_1)^2}\right\} = 0.$$

I find, (retaining the previous values of r_1, r_2,) see p. 166, that is, making $r_1 = -6{\cdot}7069$ $r_2 = 3{\cdot}0488$
$\frac{1}{\Delta_1} = -{\cdot}051265$)

$$f_2 = \frac{1}{\Delta_2} = -{\cdot}25024 \qquad f_4 = \frac{1}{\Delta_4} = -{\cdot}10023$$

from the conditions of achromaticity and the equation to determine f_3, after the numerical substitutions, is

$f_3^2 + \cdot 40766 f_3 = -\cdot 01377$, which equation gives

$f_3 = -\cdot 03716$ or $-\cdot 37050$.

From $f_3 = -\cdot 03716$, I find

$$r_3 = 3\cdot 0573 \quad \text{and} \quad r_4 = 14\cdot 151,$$

instead of $r_3 = 3\cdot 0640$ and $r_4 = 14\cdot 297$, which were the values of those quantities when the thickness of the double-convex lens was neglected. The difference in the values of the radii corresponds in each case to the indication ·0007 inch by a spherometer of 5-inch radius, which might be appreciable in large telescopes, but not, I apprehend, in common instruments.

In the last example I have neglected the interval between the lenses: if this be taken into account and the thicknesses of the lenses be neglected, it is evident that for parallel rays

$$\frac{1}{\Delta_1} = \frac{m-1}{m r_1} + \frac{y^2}{2\Delta_1^3 (m-1)^2}$$

$$\frac{1}{\Delta_2} = -\frac{m-1}{r_2} + \frac{m}{\Delta_1} + \left\{\frac{1}{\Delta^2} - \frac{1}{m\Delta_1}\right\}\left\{\frac{1}{\Delta_2} - \frac{1}{\Delta_1}\right\}^2 \frac{m^2 y^2}{2(m-1)_3}.$$

If the interval between the lenses be called t_2,

$$\frac{1}{\Delta_3} = \frac{m'-1}{m' r_3} + \frac{1}{m'(\Delta_2 + t_2)}$$

$$+ \left\{\frac{1}{m'\Delta_3} - \frac{1}{\Delta_2 + t_2}\right\}\left\{\frac{1}{\Delta_3} + \frac{1}{\Delta_2 + t_2}\right\}^2 \frac{m'(\Delta_2 + t_2)^2}{2\Delta_2^2 (m'-1)^2} y^2$$

$$\frac{1}{\Delta_4} = -\frac{m'-1}{r_4} + \frac{m'}{\Delta_3}$$

$$+ \left\{\frac{1}{\Delta_4} - \frac{1}{m'\Delta_3}\right\}\left\{\frac{1}{\Delta_4} - \frac{1}{\Delta_3}\right\}^2 \frac{m'^2 (\Delta_2 + t_2)^2}{2\Delta_2^2 (m'-1)^2} y^2.$$

The condition of achromaticity gives

$$\frac{1}{\Delta_4} - \frac{1}{\Delta_2} = -\frac{\varpi}{\Delta_2} - t_2 \left\{\frac{1}{\Delta_2^2} - \frac{1}{\Delta_3^2}\right\}.$$

Retaining the previous curvatures of the flint lens, and the values of $\frac{1}{\Delta_2}$ and $\frac{1}{\Delta_3}$ in the term multiplied by t_2, it is easy to calculate the influence which the consideration of the interval between the lenses has upon the curvatures which should be given to the double-convex lens.

$$\frac{1}{\Delta_2} = \frac{1}{1-\varpi}\left\{\frac{1}{\Delta_4} + t_2\left(\frac{1}{\Delta_2^2} - \frac{1}{\Delta_3^2}\right)\right\}$$

$$f_1^2 - \frac{(2m+1)}{m+2} f_2 f_1 + \frac{m}{m+2} f_2^2$$

$$+ \frac{m'^2 (m-1)^2 (\Delta_2 + t_2)^2}{f_2 (m+2) m (m'-1)^2 \Delta_2^2}\left\{\left(\frac{1}{m'\Delta_3} - \frac{1}{\Delta_2 + t_2}\right)\left(\frac{1}{\Delta_3} - \frac{1}{\Delta_2 + t_2}\right)^2\right.$$

$$\left. + \left(\frac{1}{\Delta_4} - \frac{1}{m'\Delta_3}\right)\left(\frac{1}{\Delta_4} - \frac{1}{\Delta_3}\right)^2\right\} = 0.$$

Supposing $t_2 = \cdot 04$, or about half an inch, $\frac{1}{\Delta_4} = - \cdot 1.$

I find $\frac{1}{\Delta_2} = f_2 = -\cdot 24389$ $\qquad \frac{1}{\Delta_3} = f_3 = -\cdot 03494$;

and after the numerical substitutions the equation for determining f_1 becomes

$$f_1^2 - \cdot 28015 f_1 = -\cdot 012113,$$

which equation gives

$f_1 = -\cdot 05348$, instead of $-\cdot 051265$, which was the value when the interval between the lenses was neglected.

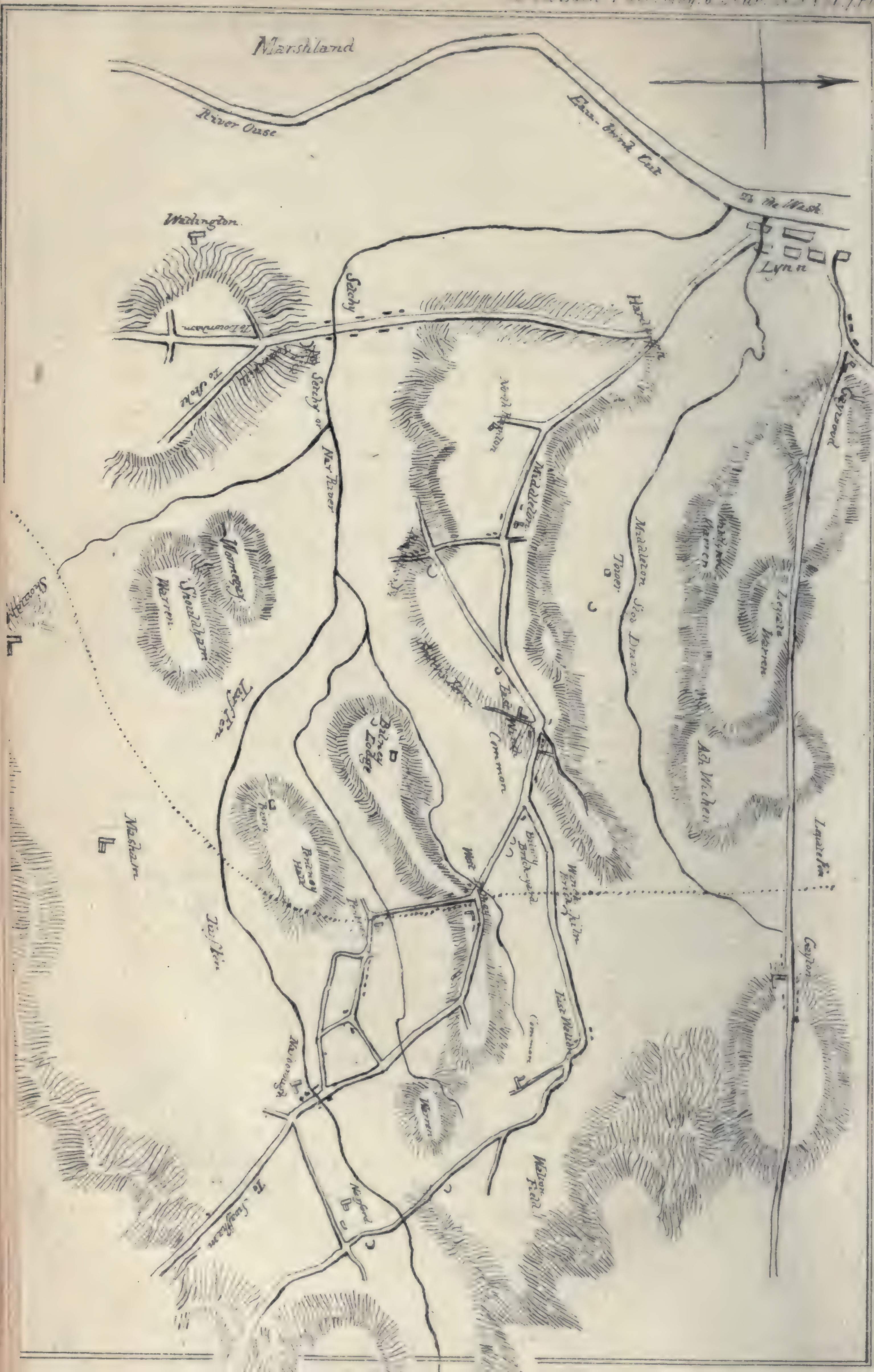
With this value of f_1, I find

$r_1 = 6\cdot 429$ and $r_2 = 3\cdot 2268$,

instead of $r_1 = 6\cdot 7069$ and $r_2 = 3\cdot 0488$. See p. 168.

It appears to result, therefore, from calculation, that the interval between the lenses influences materially the performance of the telescope, which indeed is well known to be the case practically. In consequence it would, I think, be desirable to possess a table similar to Sir John Herschel's Table 4. Phil. Trans. 1821, p. 261, calculated for some given interval between the lenses, as half an inch, for instance; such a table, combined with that which Sir John Herschel has given, would afford the means of interpolating for any interval likely to occur in practice, and hence of obtaining in any case accurate values of the curvatures which ought to be given to the four surfaces of the lenses which compose the object glass.

In p. 163, I have left the term multiplied by y^4, with a view to facilitate the appreciation of the magnitude of this quantity in the amount of aberration. In order to ascertain what aperture a telescope will bear before this term introduces confusion into the image, it would be necessary previously to know what variation in the focal length (or aberration) the eye


Marshland
River Ouse
To the Wash
Lynn
Setchy
Middleton
Middleton Tower
Bilney Lodge
Common
Ash Wicken
Warren
To Swaffham

takes cognisance of. In a telescope of ten feet focal length and an aperture of one foot, a variation of a hundredth of an inch in Δ_4 corresponds to ·0011 in the coefficient of $\frac{y^4}{8}$ in the expression for $\frac{1}{\Delta_4}$, and to ·00006 in the coefficients of $\frac{y^3}{2}$.

XX. *A Sketch of the Geology of West Norfolk. By* C. B. Rose, *Fellow of the Royal Medical and Chirurgical Society of London.**

THE geological structure of West Norfolk has received but little attention: it was first examined by the celebrated Mr. William Smith, many years since, during his professional engagements in the county. The result of his researches was made public by the appearance, in 1819, of his geological map and section of the county; and were I not in this place to bear willing testimony to the accuracy with which he has laid down the course of the chalk range, and the outcrop of the subjacent strata, I should be committing an act of injustice to one who, from the originality of his views and the great talents for observation he has displayed, is by universal consent acknowledged to be the father of modern geology.

Some distinguished members of the Geological Society have at different times visited this portion of the county, the chief object of attraction having been the cliff at Hunstanton, the only natural section of the regular strata which we possess. The information resulting from these visitations has been, I believe, (not having been able to find anything further,) confined within the limits of a note by the Rev. W. D. Conybeare, describing Hunstanton Cliff, in the 'Outlines of the Geology of England and Wales'; a description and section of that cliff by Mr. R. C. Taylor in the Philosophical Magazine for February 1823 (vol. lxi. p. 81); and a notice of the chalk of West Norfolk, in a paper read before the Geological Society, May 2nd, 1823, by the same gentleman, and published in its Transactions. There is also a brief notice of the strata, with a list of the *organic remains*, in the 'Outlines of the Geology of Norfolk', published in 1833, by Mr. Samuel Woodward of Norwich.

The observations which follow are offered as contribution towards the completion of a task, which, I trust, at some future time will be undertaken by those who are more competent, and have more leisure than myself.

* Communicated by the Author.

The area of my inquiries has the natural boundaries of the county to the south, west, and north, and a line drawn from Wells to Thetford traces its eastern limit. The regular strata comprised within this area are, the chalk, with and without flints; the chalk-marl, including the firestone; the gault; the inferior greensand; the Kimmeridge and Oxford clays.

Taking Mr. Smith's geological map for my ground-plan, I propose to furnish also some notices of the subordinate members of the strata he has represented which were unknown to geologists at the period of his inquiries. Before entering upon the strata in detail, I will insert a tabular arrangement of them, in their order of superposition, beginning with the lowest member of the series.

Formations.	*Subdivisions.*	*Localities.*
Oolite	Oxford clay	Marshland and the Fens.
	Kimmeridge clay, inclosing Septaria, and beds of bituminous shale	Southery, Gaywood, &c.
Greensand	Inferior greensand, carstone (provincial)	Hilgay, Downham, Shouldham, Middleton, Hunstanton, &c.
Chalk	Gault, golt brick-earth of Smith	West Dereham, Shouldham, Pentney, Bilney, &c.
	Red limestone, or gault equivalent	Hunstanton, Heacham.
	Chalk without flints, including the firestone equivalent, and chalk-marl	Hockwold, Stoke Ferry, W. Dereham, Shouldham, Marham, Gayton, Sandringham, Hunstanton.
	Chalk with flints, or medial chalk of Woodward	Thetford, Saham, Swaffham, Castleacre, Litcham, Thornham, Burnham, &c.
Diluvium	Yellow and blue clay, brick-earth loam, sand, gravel, coarse breccia, &c.	General covering.
Alluvium	Marine and lacustrine deposits, peat, subterranean forests, and calcareous tufa	Valley of the Nar, Eau-brink, Marshland, and the Fens, Stoke, Marham, Carbrook, &c.

From the above list of the strata, it will be seen, that several other members of the British series, usually found associated with ours, are wanting, namely, the coral rag, Portland oolite, Purbeck limestone, Hastings sand, and Weald clay; and it is probable that no equivalent of either of them exists; for although I have not been so fortunate as to see the inferior greensand immediately reposing upon the Kimmeridge clay, the gault upon the inferior greensand, or the lower chalk and marl beds upon the gault, no excavation having, to my knowledge, been made at those particular junctions of the

strata, still, as the spaces at present unexplored are very *narrow*, and as the junction of the above-mentioned strata or an equivalent is exposed at Hunstanton Cliff, with the exception of that of the inferior greensand with the Kimmeridge clay, and of the latter with the Oxford, it is but a legitimate inference that my arranged list exhibits the entire assemblage of strata in the western division of our county.

A brief description of each stratum in detail I now proceed to give, with a list of its *organic remains.*

Oxford Clay.—This deep bed of clay (the clunch clay of Smith) occupies the whole of Marshland, and the fens of the Norfolk portion of the Bedford Level: it forms the great substratum of this extensive flat; but possessing no qualities offering any inducements to open it for œconomical purposes, we are in consequence but little acquainted with its physical characters and oryctological contents. The most extensive exposures of it have been at Denver Sluice and at Lynn; at the former locality I had an opportunity of examining it, and was favoured by Mr. Thornton (the clerk of the works) with the following section and admeasurements. No organic remains had been observed by the labourers: at my visit, I picked out of the clay a compressed specimen of *Ammonites decipiens.*

Section at Denver Sluice.

1. A light brown sandy loam ... ... 14, ft.
2. Peat 2
3. Blue clay, inclosing roots and small portions of peat 2
4. Peat, similar to No. 2 3
5.*Similar to No. 1, but somewhat more argillaceous 2
6. Dark ferruginous sand 3
7. Oxford clay, dark blue, and very tenacious 5½

Section at Lynn.

To Thomas Allen, Esq., I am indebted for the following detail of the strata observed in sinking a well for the supply of a brewery. The Oxford clay was penetrated to the depth of 630 feet; the first 450 were dug, the remaining 180 bored; and after all the labour and expense, the quality of the water was inapplicable, and the supply inadequate, to the wants of the enterprising proprietor.

1. Vegetable soil.
2. Loam, used for making bricks ... ... 7 ft.
3. Peat ... ... 2 to 2½
4. Blue clay 8

* Immediately beneath the peat on the surface of No. 5, a farthing of Charles II. and a pair of scissors were found.

5. Peat, with alder and hazel bushes		2 to 3 ft.
6. Blue clay, with *marine* silt, containing testaceous exuviæ.	about	30
7. Blue clay, inclosing nodules of chalk		
8. Oxford clay, with Septaria, &c.		630

A small portion of the upper part of stratum 7. at Denver, and 8. at Lynn, may possibly be the Kimmeridge clay, but we found no fossils to determine that point. The bed of clay (No. 7.) at Denver was very stiff and tenacious, and parted into thick laminæ upon drying: throughout the few feet that were exposed, I could discern neither shale nor Septaria.

Specimens of the organic remains taken from the lower beds of the clay in Mr. Allen's well were liberally presented to me by that gentleman; the following is a list of them:

Name.	Reference.
Ammonites decipiens	Min. Conch., tab. 294.
——— excavatus	——— tab. 105.
———	very small, species undetermined.
Belemnites abbreviatus... ...	Min. Conch., tab. 590, fig. 9.
Gryphæa bullata	——— tab. 368.
Serpula tricarinata, attached to the Gryphæa	——— tab. 608, fig. 3.
Mya depressa...	——— tab. 418.

Venus? a fine cast in *iron pyrites*, with the impression of a Pecten upon it.

Muricated spine of an Echinus.

Kimmeridge Clay.—This bed forms a narrow belt to the escarpment of the superincumbent *inferior greensand*, and is the intermediate stratum to that, and the Oxford clay beneath, which rises to the west of it, but is hidden by the alluvial strata of Marshland and the fens. Smith has laid down its situation and course upon his map correctly, but has erred in stating that it is "the deepest stratum in the county."

We have had no opportunity of ascertaining the thickness of this bed, for there is no natural section of it in the county; indeed, if there were, it would probably prove a difficult task (the coral rag beds not intervening) to determine where the Kimmeridge terminated and the Oxford clay commenced. Its surface has been exposed at Southery* to a considerable extent, but not pierced sufficiently to aid us in our inquiry respecting its thickness. Last summer I visited a brick-yard in that parish, and was informed by the brickmaker, that upon the removal of thirteen feet of brick-earth, he exposed a floor

* In Mr. Woodward's map this locality is erroneously coloured, the inferior greensand being represented.

of shale two or three inches in thickness, and he traced it for thirty yards or more. A specimen of the shale in my possession burns readily, crepitating like cannel coal: between its laminæ impressions of an Ammonite and a small Tellina? are seen. This clay has also been opened at Gaywood near Lynn, in sinking a well fifty feet in depth: the first eighteen feet were sand, succeeded by fourteen inches of blue clay; then followed a laminated clay containing *Septaria*, which continued to the depth sunk: no organic remains were preserved. The *characteristic* shell of this stratum is the *Ostrea deltoïdea*; I am not certain that it was met with in Norfolk by Mr. William Smith. I possess two specimens, but they were found in clay (diluvium) to the *east* of the outcrop of the *greensand*, and were therefore transported specimens. The following fossils are from the diluvial clay covering the Kimmeridge at Stow Bardolph: alveolus of a Belemnite; Pecten *cinctus* of Min. Conch., tab. 371; cast of a Trigonia in pyrites; cast of an Astarte? in pyrites; paddle-bone of a Plesiosaurus; tooth of a Ruminant; wood impregnated with brilliant yellow pyrites; bituminous shale very pyritous; Septaria, and blocks of a breccia composed of black pebbles and pyrites in a calcareous matrix: similar breccia and Septaria may be seen *in situ* in the Kimmeridge clay at Ely in Cambridgeshire.

Inferior Greensand.—This formation consists of alternating beds of ferruginous sand, sandstone, and white sand. It is on Smith's map denominated "Sand beneath the golt brick-earth, in the lower part of which the Portland rock is occasionally found." It is provincially called Carstone, and presents little of interest to the naturalist, being a mere mechanical deposit, and, as far as it has at present been explored, affording no animal remains.

It occupies the high ground between the Kimmeridge clay and the chalk range, extends from Hunstanton on the north to Hilgay on the south, and in some parts of its course rises into hills vying in altitude with those of the lower chalk; their western declivities are abrupt. Its course is faithfully represented by Smith on his map; but I am at a loss to understand where he "occasionally found" the Portland rock. When *he* wrote, the divisions of the greensand formation had not been ascertained as at the present time; at that period this member was confounded with the Hastings sand, which reposes upon the rocks of the *upper* division of the oolites: it is therefore probable he meant that when the series of strata is complete, the Portland rock lies beneath the sand, and not that it really occurs in Norfolk.

This formation is an aggregate of siliceous particles of va-

rious magnitudes, and ferruginous matter cemented into beds of sandstone through the agency of aqueous infiltration, and separated from inferior beds of similar character by a considerable mass of loose white sand. In the lower beds exposed at Hunstanton Cliff many of the quartzose pebbles are sufficiently large to give the rock the character of a breccia. The loose beds occasionally inclose thin strata of fullers' earth and a tenacious green clay*: its upper portion is invariably ferruginous, occurring in thin layers of hard sandstone, or tabular masses containing numerous veins of ironstone.

Extensive portions of the surface of these sands are extremely sterile, as at Dersingham Heath, Castle Rising, Bawsey, Ashwicken, Blackborough in Middleton, and Shouldham Warren.

In all parts of the sandstone range occur springs of water with a chalybeate impregnation, but the fame of their medicinal virtues has not extended beyond their immediate vicinity. I cursorily examined the water of Gaywood near Lynn, and ascertained that it resembled the Tunbridge, but did not retain so much free carbonic acid.

The dip of these beds is about five yards in the mile; and I consider their entire thickness to be about eighty feet: their surface width averages nearly three miles.

For a building-material the carstone is invaluable; the noble stables at Houghton Hall are built of it: "our ancestors formed it into querns or corn-mills†."

As these sands exhibit peculiarities at different localities, I will notice a few that I have met with, beginning with Hunstanton. In the cliff is exposed a section of this formation between forty and fifty feet in depth: the upper two feet resemble the consolidated *carstone* usually found above the more friable and light-coloured bed, which at this spot immediately succeeds it to the depth of eight feet, and has a greenish shade of colour; then follows a *breccia*, more than thirty feet in thickness, horizontally and vertically divided into tabular masses of considerable magnitude by a loosely aggregated sand: numerous veins of white spathose matter not thicker than wafer-paper run vertically through the *breccia* at irregular distances from each other. Great numbers of blocks of the *breccia* are seen upon the beach occupying their *original* site, the more friable materials of the cliff having been removed by the waves of the ocean. On my visit to the cliff last summer, Mr. Edward Muggridge, of Lynn, satisfactorily proved to me that these *masses* retained their original

* The colouring ingredient is *silicate of iron*.

† Geology of Norfolk, p. 30.

situation, by pointing out the parallelism of the vertical lines of spathose matter in *them* and in the *present* cliff. It has not been determined to what depth the *breccia* extends, but it has been stated that the clay *beneath* is exposed at low water.

At Dersingham the lower portion of the sand exposed is colourless, nearly pure silica, sufficiently so for the use of the glass manufacturer, and is shipped in large quantities for the glass furnaces in the north.

Between the castle and the water-mill at Castle Rising, I observed large blocks of sandstone lying by the road-side, apparently removed from the loose sand dug for domestic purposes: they varied in colour from light ash to nearly a black.

At Middleton, to the left of the road leading from East Winch to Setch, a large quarry is worked for building-stone, called the Blackborough Car-pit; it is opened to the depth of twenty feet, and the stone is highly ferruginous, overlying the loose variegated sand which comes to the surface at the base of the hill towards the west. At this locality the carstone is intersected by innumerable veins of *ironstone* from one eighth to an inch in thickness; they run horizontally and vertically, forming grotesque lines, all tending to a concentric arrangement: the veins have a dark-brown surface; the fracture is granular, probably from an intermixture of sand, and has a semi-metallic lustre. The carstone is very friable when first raised, but hardens by exposure, and becomes an imperishable building-stone.

At West Bilney, carstone of a similar quality to the last mentioned is quarried in a pit adjoining the garden of Bilney Lodge.

Section of the Pit.

Vegetable soil and loam, varying from... ...	2 to 4 feet.
Rubble car	2
Regular car of very loose texture	5
Ironstone irregularly distributed, but horizontally	1 to 2 ines.
Friable sand, upper portion ferruginous, lower portion green, containing thin tabular carstone and blocks of the same; also geodes and hollow cylinders of ironstone filled with sand	4 feet.
Car with thin veins of ironstone containing a little clay	4 to 6 inches.
Hard carstone	6 feet.

The hard carstone appears to be divided into layers, or strata, varying in thickness from nine inches to thirty, some separated by very thin layers of sandy clay, and other divi-

sions appearing to be only a horizontal separation of one thick stratum into two or more without any intervening substance; there are also nearly vertical divisions of the rock, cutting it into irregularly formed rhomboidal blocks.

A few hundred yards to the south of the above pit, a well has recently been sunk, affording very interesting information: the following is a list of the strata, communicated to me by the labourer, an intelligent man, and accustomed to the employment.

	Ft.	Inch.
1. Ferruginous sand, containing geodes and hollow cylinders of ironstone	7	0
2. Carstone	8	0
3. White sand with occasional brown veins ...	7	0
4. Green clay	0	6
5. Carstone, darker than upper beds	2	0
6. Carstone, with six or seven partings of the green clay 1 to $2\frac{1}{2}$ inches in thickness, not in regular courses, but the stone imbedded irregularly in the clay	3	0
	27	6

This section is particularly valuable, as it identifies our sands with the inferior greensand of Sussex*.

At Shouldham Warren, an extensive excavation exposes the variegated sands to the depth of twelve feet, covered by three or four feet of rubbly carstone and ferruginous sand: the varieties of sand here are white and brown, thin veins of the latter running horizontally through the former, and frequently assuming a concentric form. In the rubble at this spot, as at most other localities, geodes, containing sand, small hollow cylinders, and flat fragments of ironstone, are very abundant, evidently resulting from the disintegration of that portion of the ferruginous sand containing the veins of ironstone, the concentric arrangement of which has produced those forms.

At Downham, the loose sand is opened to the depth of fifteen feet; it is covered by two or three feet of thin tabular carstone. The general mass of the sand is white, with veins of a greenish colour, and also a few yellow veins: six feet from the surface there is a stratum of *fullers' earth* about an inch in thickness, of an ash colour, with ochry specks interspersed; its analysis gives an abundance of iron in its composition, with a very slight trace of carbonate of lime. The ferruginous sands again appear from beneath their diluvial covering at West Ryston; further south they are hidden by the alluvial

* Phil. Mag. and Annals, N.S., 1827, vol. i. p. 138. Dr. Fitton.

deposits of the fens, and have been seen at Hilgay only, upon sinking wells.

A section from Bilney Lodge to Blackborough, cutting the *strike* of the inferior greensand at right angles, would exhibit the following succession of beds, beginning with the uppermost:

1. The six beds exposed at the well near Bilney Lodge.
2. Loose sand, occurring in the low ground of Bilney Common.
3. Carstone, extending from East Winch to Blackborough Pit.
4. Loose variegated sand, appearing at the base of the hill in which the above pit is situated, and towards the west.

Mineral Contents.—In addition to the ironstone already mentioned, we meet with a form of iron *pyrites* very unlike that from the chalk strata above, or the Kimmeridge clay beneath; its form is spherical, outer coat brown, and its fracture granular, with a very light pyritous lustre: it is rarely seen. Titaniferous oxidulated iron was found at Hunstanton by Mr. Aikin. I have not at present discovered any chert.

No *animal remains* have yet been found. Small fragments of wood are occasionally found imbedded in the carstone; and Mr. Muggridge gave me a portion of sandstone from Snettisham, having on it an impression of the stem of a Lycopodite?.

Gault.—This bed, the "golt brick-earth" of Smith, succeeds the inferior greensand, and reposes upon it. It occupies, with the lowest beds of the chalk above, the valley between the ranges of chalk and greensand, emerging from beneath the western edge of the former.

Upon referring to the map and section in the 'Outlines of the Geology of Norfolk', it will be seen that this stratum is omitted; in justice to Mr. Woodward, I here state, that I believe I am responsible for that omission, that gentleman having probably relied upon my local knowledge of Western Norfolk. At the time I communicated with him, I entertained great doubt of there being a regular stratum of *gault* in this county, not having met with any other fossil than the small Belemnite in the "golt brick-earth" of Smith,—nor, indeed, have the other *characteristic* fossils* of the "gault of Cambridgeshire" yet been found; but when I had the good for-

* *Inoceramus concentricus* and *Inocer. sulcatus.* Since writing the above, I have ascertained that *Inocer. sulcatus* has been found in the red beds at Hunstanton.

tune to see Mr. Smith at the meeting of the British Association at Cambridge, he so positively assured me of the existence of the gault that I no longer questioned such high authority, and on my return home redoubled my inquiries: fortunately, two wells were sunk soon after my return, near to one of the localities marked on Smith's map, and from them I have assured myself that the gault exists as placed in his map and section. Mr. Smith has laid down but a few detached spots, those only where he had *seen* it; had he extended those patches into one continuous line, he would have correctly traced nearly its whole course: such adherence to ocular demonstration greatly enhances the authority of this veteran in geology.

A nearly continuous valley, situated between the outcrops of the chalk and lower greensand, extends from Wretton, through West Dereham, Shouldham, Marham, Pentney, Bilney, Gayton, Grimstone, to Congham, and along this valley I have traced the gault the entire distance, with the exception of one spot at Stradsett, where it is hidden by a large accumulation of diluvium, which here destroys the continuity of the valley. At West Newton this valley terminates, or, rather, becomes a *transverse* valley, taking its course westward through the sand hills; from this place to Hunstanton the chalk and lower greensand occupy the same ridge: no valley intervenes, an indication of the absence of a soft stratum between them, as exhibited at Hunstanton Cliff, where the red chalk is interposed between the two. Professor Sedgwick says, the red beds of Hunstanton are "exactly in the place of the gault of Cambridge, and contain some of the gault fossils, for example, the *Belemnites Listeri*;" such is the fact, and I fully concur with the distinguished Professor in considering the red beds the equivalent of the gault.

The two wells before mentioned sunk in the gault at Bilney and Pentney, about a mile apart, and in the direct line of its *strike*, afford the following section:

Pentney.	Ft.	Inch.
1. Vegetable soil and gravel	4	0
2. Rubble chalk, succeeded by stiff blue clay, containing *Belemnites minimus*; the "golt brick-earth" of Smith	10	0
3. Hard gray limestone, containing *Terebratulæ biplicatæ* resembling those in the red chalk of Hunstanton	0	6
4. Blue clay or gault, very tenacious	2	0
5. Black sand	1	0

The well at the school-house at Bilney passed through the same series of strata, but at this locality the gray limestone inclosed *Inoceramus gryphæoïdes* and *Belemnites minimus*; the clay contained the same Belemnite, and also *Belemn. attenuatus.* From an old clay pit near Pentney church, I took the above two species of Belemnites: this is, I believe, the spot referred to by Smith on his map; the gray limestone has not been exposed at this place.

I shall here describe the red chalk of Hunstanton as the equivalent of the gault. It is a compact limestone coloured by oxide of iron, containing many small dark-green siliceous pebbles, and is divided into two beds: the uppermost, about seven inches in thickness, abounds in organic remains; this bed is intersected throughout by a ramose Zoophite, the nature of which is not satisfactorily determined; and the two characteristic species of Belemnites are in great abundance; *Terebratula biplicata* and *Inocerami* are numerous, and one species of *Nautilus* occurs. The lower bed is three feet five inches in thickness, contains less of the Zoophite, and fewer fossils than the upper, but the siliceous pebbles are more numerous. I have not been able to trace these beds inland beyond Heacham.

A seam of dark red argillaceous matter, two to three inches in thickness, separates these beds from the incumbent white beds. Upon analysis it proves of the nature of fullers' earth, so highly coloured with oxide of iron that it has been used as a pigment.

The paucity of genera among the *organic remains* at present discovered in this stratum is not to be wondered at when we compare its thickness with that of the Cambridge gault. With us little more than fifteen feet* is probably its utmost depth: at Cambridge it is said to be 150 feet: but it must not be supposed that all its contents have been seen, for but few perforations have been made where any attention has been paid to its contents.

The gault of Norfolk affords a remarkable example of dissimilarity in the mineralogical character of adjoining portions of a contemporaneous deposit, and is an additional illustration of the necessity for employing the zoological character to determine their identity. Without consulting these "medals of Nature," who could imagine that the *red limestone* of Hunstanton Cliff, and the *blue gault* containing *gray limestone* at Pentney and Bilney, were deposits of the same epoch?

* The boring of a well at Diss extends it to twenty feet. [See a notice of Mr. John Taylor's paper on the strata penetrated in sinking a well at Diss, in Lond. and Edinb. Phil. Mag., vol. v. p. 295.—Edit.]

Organic Remains.

Name.	*Reference.*	*Locality.*
Spongia paradoxica......	Geol. of Norfolk.	Hunstanton.
RADIARIA.		
Apiocrinites ellipticus*.	Miller, p. 33.	Hunstanton.
ANNELIDES.		
Vermicularia umbonata.	Min. Con., t. 57. f. 6, 7.	Ditto.
Serpula pentangulata....	Geol. Norf., t. 5. f. 17.	Bilney.
CONCHIFERA.		
Inoceramus Cripsii.......	Geol. Suss., t. 27. f. 11.	Hunstanton.
——— striatus	Min. Con., t. 582. f. 2.	Ditto.
——— tenuis	Geol. Suss., p. 132.	Ditto.
——— gryphæoïdes	Min. Con., t. 584. f. 1.	Bilney, Congham.
——— sulcatus ...	Ibid. t. 306.	Hunstanton.
Exogyra haliotoïdea?...	Ibid. t. 25.	Ditto.
Pecten Beaveri.	Ibid. t. 158.	Ditto.
Terebratula pentangulata.	Geol. Norf., t. 6. f. 10.	Ditto.
——— intermedia.	Min. Con., t. 15. f. 8.	Ditto.
——— ovata.	Ibid. t. 15. f. 3.	Ditto.
——— biplicata....	Ibid. t. 90.	Hunstanton, Pentney.
——— plicatilis....	Ibid. t. 118.	Hunstanton.
——— rigida.	Ibid. t. 536. f. 2.	Pentney.
MOLLUSCA.		
Belemnites minimus	Min. Con., t. 589. f. 1.	General.
——— attenuatus,	Ibid. t. 589. f. 2.	Ditto.
Nautilus elegans†.	Ibid. t. 116.	Hunstanton.
Ammonites varians.......	Ibid. t. 176.	Ditto.
——— lautus.	Ibid. t. 309.	Bilney.
——— dentatus. ...	Ibid. t. 308.	West Dereham, Hunstanton.
———‡ undetermined	....	Congham.
Hamites raricostatus. ...	Smith's brick-earth, f. 2.	Near Grimstone.

[To be continued.]

XXI. *On the greater calorific Effect of the Sun's direct Rays in high than in low Latitudes.* By JAMES DALMAHOY, *Esq., Surgeon, Hon. R.I.C.S.*§

IT is well known that experiments have been adduced to prove that, at equal altitudes of the sun, the heating power of its direct rays is greater in high than in low latitudes.

The following is an attempt to reconcile this result with the received theory of radiant heat.

MM. Dulong and Petit have discovered that if the initial

* Portions of the column.

† May be *Nautilus simplex* of Min. Con., t. 122.

‡ A similar species is found in the gault at Cambridge.

§ Communicated by the Author.

intensity* of radiant heat emanating from a particular surface at the temperature 0° be expressed by i, the intensity at any other temperature, t, will be expressed by $i a^t$; the quantity a being constant for the same thermometric scale, and i for surfaces having the same radiating power.

Hence it may be concluded that if radiant heat, at whatever distance from its source, have an intensity expressed by $i a^x$, it is equal to the *initial* intensity of heat emanating from a surface the radiating power of which is i, and its temperature x.

Again, suppose the bulb of a thermometer radiating heat of a given intensity, $i a^t$, to be exposed during a short period, such as a minute, to the influence of solar heat of some superior intensity, $i x^x$, and that the velocity of heating, as measured by the number of degrees which the thermometer rises during the given time, is represented by v, then, according to Dulong and Petit †, the relation between these quantities may be expressed by the equations

$$v = m\,(a^x - a^t) \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

$$a^t + \frac{v}{m} = a^x, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

where m is constant for the particular instrument by which v is measured.

From these equations it follows:

1st, That the intensity of the sun's rays being the same, their calorific effect ought to increase as the temperature of the air diminishes.

2ndly, That the intensity of the sun's rays is proportional not simply to the increment of temperature which they impart to a thermometer in a given time, but to that increment added to the quantity which expresses the intensity of the thermometer's radiation.

But there are two objections to which these equations may, perhaps, be thought liable, namely,

1st, That the quantity a^t, being small compared with a^x, may be neglected, and both equations be reduced to the form

$$v = m a^x \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

2ndly, That in the particular case when v becomes $= 0$, the intensity of radiation is still expressed by

$$a^t = a^x.$$

* The terms "intensity" and "density" may here be regarded as convertible.

† When v is determined by Sir John Herschel's very beautiful and simple method, every condition supposed in the reasoning of MM. Dulong and Petit is complied with.

In reply to the former objection it may be remarked, that as x may have every magnitude from its maximum down to the value of t, it follows that the ratio of a^t to a^x must be *frequently* very large, and consequently that equation (3.) cannot without error be substituted for equations (1.) and (2.).

The answer to the other objection seems to be, that as the effect of the sun's direct rays cannot be separated from the effect of the *parallel* rays mingled with them, but proceeding from the sky and intervening clouds, it is necessarily the united intensity of these that is measured. But as the united intensity of these, while the sun is above the horizon, can never be less than the thermometer's radiation in the opposite direction, it follows that a^x can never be less than a^t even when v is $= 0$.

I shall now quote one or two of the experiments on which the proof of the greater calorific effect of the sun's rays in high latitudes rests; and then endeavour to show that their results are not at variance with the conclusions which have been already deduced from the received theory of radiant heat.

In the appendix to Captain Franklin's narrative of a second expedition to the shores of the polar sea, there is a valuable paper by Dr. Richardson on solar radiation, from which the following remarkable passage is extracted:

"The amount of solar radiation (at Fort Franklin) shown by Leslie's photometer was regularly noted in March and May, and occasionally in the other months; *but when the temperature of the air was low, and the sun bright*, the coloured liquid was frequently driven entirely out of the limb of the instrument to which the scale is attached, and in twelve different instances in the month of March, the whole of the liquid was forced into the colourless bulb. Whether this was owing to the instrument not being calculated for measuring greater solar radiation when the temperature of the air was very low, or to a defect in its construction attributable to the maker, I am unable to say, but as the results could only be guessed at after the liquid had descended below the scale, I have not inserted them in the tables. In May the liquid in the photometer seldom entirely left the stem to which the scale is applied, and in general in that month a degree of its scale corresponded to a greater number of degrees of the blackened thermometer than in March."

It appears from this very explicit testimony that a greater calorific effect of the sun's rays was *frequently* observed at Fort Franklin in latitude 65° 12′, than was *ever* observed at London in latitude 51° 31′. But it is also worthy of remark, that at Fort Franklin the sky was clearest in March,

about the vernal equinox, when the meridian altitude of the sun does not exceed 24° 48′, and the mean maximum temperature of the air is 3°·9; whereas in London the sky is clearest in June, about the summer solstice, when the meridian altitude of the sun is 61° 57′, and the mean maximum temperature of the air is 69°·4. Now, according to Sir John Leslie*, if the atmosphere be equally clear, the number of rays which reach the earth when the altitude of the sun is 61° 57′, is to the number when the altitude is 24° 48′, nearly in the ratio of 725 : 507. Hence it may be concluded that when the temperature of the air was very low, 507 rays produced a greater calorific effect than 725 rays when the temperature of the air was high.

The following experiment, though less striking, leads to the same conclusion.

By the observations of Professor Daniell in London, and of Dr. Richardson at Fort Franklin, it appears that at both places the maximum effect of the sun's rays upon a blackened thermometer was 65 degrees† of Fahrenheit's scale. But in this as in the former experiment, the observation at Fort Franklin was made in March, while that at London was made in June; it follows therefore, according to the rude method of approximation already adopted, that the calorific effect of the sun's rays when the temperature of the air is 69°·4, is to the effect when the temperature is 3°·9 in the ratio of $65 : 65 \times \frac{725}{507}$ or 65 : 89.

It would have been desirable to have been able to compare with the foregoing observations of Professor Daniell and Dr. Richardson those of Captain Sabine in tropical climates. But Captain Sabine's observations having been made (at least those at Sierra Leone, and probably also those at Bahia,) during the prevalence of a cooling wind, they cannot be properly referred to as indicating the *maximum* effect of the sun's rays. Still, when compared with *similar* observations in higher latitudes, they strongly confirm the result already derived from a comparison of the sun's heating power at London and Fort Franklin.

But there are some circumstances respecting the foregoing experiments which may seem to detract from their value, or to point to a different conclusion from that which has been drawn from them.

* Supplement to the *Encyclopædia Britannica*, article CLIMATE.

† The maximum effect observed by Captain Back and Lieut. Kendall, at Fort Franklin, was on the 1st of April, and amounted to 70 degrees; but as the thermometer was sheltered by a glass case, and therefore not under the same circumstances with Professor Daniell's instrument, Dr. Richardson's maximum is adopted.

1st, Dr. Richardson remarks that the indication of the photometer bore a larger ratio to that of the blackened thermometer in March than in May, and this may, perhaps, suggest a doubt as to the accuracy of one or both of these instruments. But if it be considered that the photometer is *directly* affected only by solar radiation, while the blackened thermometer is influenced both by solar and terrestrial radiation, it will appear probable that the variation alluded to arose from no defect, at least in the photometer.

2ndly, Further, the apparent greater effect of solar radiation at Fort Franklin may be ascribed to the reflection of indirect rays from the snow. But as observation seems to prove the effect of the sun's rays to be greater in temperate than in tropical climates independently of the presence of snow, it follows that the cause assigned above is not sufficient to account for the whole difference of effect at Fort Franklin. Moreover, judging from the effect of the sun in melting the snow when the temperature was much below the freezing point, there is reason to doubt whether snow, as the objection assumes, reflects the heating rays in great quantity*.

Since, therefore, the experiments which have been adduced (though less satisfactory than if they had exhibited the momentary effect of solar radiation) seem liable to no important objection, it may be fairly inferred from them that,. at equal altitudes of the sun, the heating power of its direct rays is greater when the temperature of the air is low than when it is high,—a conclusion the same as has been already drawn from the equation $v = m\,(a^x - a^t)$.

But this result will necessarily appear anomalous when tried by the equation $v = m\,a^x$, and can only be reconciled with it by assuming that in high latitudes, either the atmosphere is more pervious to the sun's rays, or the power of bodies to absorb radiant heat is greater, than in low latitudes.

The grounds upon which Professor Daniell has supported the former of these hypotheses, I believe, are now generally allowed to be untenable†. Dr. Richardson has suggested that at Fort Franklin, in the month of March, the air may be rendered more transparent than in lower latitudes, by containing much less than its due quantity of moisture. With respect to this explanation it may be remarked, that in a tropical

* Regarding the effect of colour, see article CLIMATE, p. 194. [See also Dr. Stark's researches on the Influence of Colour on Heat, Phil. Trans. 1833, p. 285. *et seq.*, of which an abstract will be found in Lond. and Edinb. Phil. Mag., vol. iii. p. 458.—EDIT.]

[† We observe, however, that Mr. Royle appears to subscribe to this hypothesis: see our last Number, pp. 135, 136.—EDIT.]

climate, shortly after the rainy season, the transparency of the atmosphere is so perfect as to render it difficult to admit, except on experimental proof, that the air of higher latitudes can be more pervious to the sun's rays.

Admitting, however, that at Fort Franklin the increased effect of the sun's rays was entirely owing to a greater transparency of the atmosphere, the same cause would necessarily have produced a corresponding increase of terrestrial radiation. But so far was this from being the case, that Dr. Richardson never saw the effect of terrestrial radiation exceed 4 degrees at Fort Franklin, while in London Professor Daniell has observed it amount to 17 degrees. Another fact at variance with the same hypothesis is, that in Bengal the effect of terrestrial radiation is frequently sufficient to freeze water when the temperature of the air is considerably above 32°. I am aware that the observations of Captain Sabine seemed to indicate that the effect of terrestrial, as well as of solar radiation was less in low than in high latitudes, but these observations were evidently too few to permit any conclusion being drawn respecting the *maximum* effect in either case.

But while the facts respecting terrestrial radiation are irreconcilable with the hypothesis that the atmosphere is more transparent in high than in low latitudes, they admit of an easy explanation on the principles already advanced. For, when the formula $v = m' (a^x - a^t)$ is applied to the measurement of terrestrial radiation, v is negative from t being greater than x; and as x may be supposed constant when the sky is perfectly clear, it follows that v will be greater when t is large than when it is small, therefore greater in low than in high latitudes.

The other hypothesis respecting the cause of the increased effect of solar radiation in high latitudes is, that the power of surfaces to absorb radiant heat augments with the obliquity of the incidence of the rays. This ingenious suggestion is ascribed by Dr. Prout* to Professor Daniell; but there does not appear to be any proof, except from analogy, that the power of bodies to absorb radiant heat varies in the manner assumed.

Having thus endeavoured to show that the greater effect of the sun's direct rays in high than in low latitudes is not at variance with the received theory of radiant heat, I shall conclude by offering a few remarks on the constant quantities m and a in the formula $a^t + \frac{v}{m} = a^x$.

By whatever method the increment of temperature v is de-

* See Dr. Prout's Bridgewater Treatise, p. 237.

termined*, the constant quantity m must be such as to render $\frac{v}{m}$ the same for the same intensity of radiation.

In order to determine the value of m for any instrument, suppose, while the intensity of the sun's rays continues unchanged, v to be the velocity of heating at the temperature t, and v' at some higher temperature t', there is then the equation $a^t + \frac{v}{m} = a^{t'} + \frac{v'}{m}$, by means of which is found

$$m = \frac{v - v'}{a^{t} - a^{t'}}.$$

When m has been determined for any scale, if m degrees of that scale be adopted as the unit of a new scale, it follows that V degrees of this latter scale will be equivalent to $\frac{v}{m}$ degrees of the other, and equation (2.) will, for all instruments, be $a^t + \mathrm{V} = a^x \quad . \ . \ . \ . \quad (4.)$.

If t and x are degrees of the centigrade scale, the value of a, according to Dulong and Petit, is 1·0077. But as ·0077 is a small fraction, and t, either when positive or negative, seldom exceeds 44, it follows that $a^t = (1 + {\cdot}0077^t)$ may be expressed with sufficient accuracy by the first three terms of its development, and these being substituted in equation (4.), it becomes

$$1 + {\cdot}00767 \times t + {\cdot}000038 \times t^2 + \mathrm{V} = (1{\cdot}0077)^x.$$

But if the temperature be expressed in degrees of Fahrenheit's scale, then a is equal to 1·00425, and the equation becomes

$$1 + {\cdot}00424 \times \mathrm{T} + {\cdot}0000092 \times \mathrm{T}^2 + \mathrm{V} = (1{\cdot}00425)^x.$$

As the temperatures to which these formulæ relate are comparatively small, it is of no importance whether they be measured by the air-thermometer or by one of another kind.

Edinburgh, Feb. 20, 1835.

XXII. *On* Taylor's *Theorem.* *By* A Correspondent.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THAT every proof which can be given of this theorem will meet with objections may be suspected from the circumstance of every one which has hitherto been given having

* See Professor Powell's "Report on the present state of our knowledge of the science of radiant heat," (in the Second Report of the British Association,) under the head "Measures of radiation."

been objected to; and I strongly suspect that it must finally be classed with the disputed axiom of Euclid, and other points in which the mathematics touch upon metaphysics. The proofs which have been given may be briefly classed as follows:

1. Those which are derived from the binomial theorem, to the proofs of which the above-mentioned objections equally exist.

2. Those which are derived from combined considerations of expansions and limits; which are liable to the objections urged against both methods.

3. That of Lagrange, which assumes expansions only, and to which the objections of Woodhouse and others are well known.

But I am not aware that any proof is commonly circulated which depends *upon limits only*, though the various considerations upon which I have put together the following are not unfrequently mentioned. The fundamental theorems from which it is deduced are marked I., II., and III., and the form in which the theorem is deduced is that of Lagrange, after the method of M. Cauchy in his *Leçons sur le Calcul Différentiel.* But the last-mentioned writer has not established it entirely on the theory of limits.

I now proceed to the point in question, and enunciate the several steps of the process.

Axiom, commonly assumed in what is called the *law of continuity.* If ϕx be a function which continually decreases or increases from $x = a$ to $x = b$, without becoming infinite for any value of x contained in that interval, then if $\phi a = \mathrm{A}$ and $\phi b = \mathrm{B}$, any value lying between A and B may be given to the function ϕx, by giving a value to x lying between a and b.

Theorem 1. If the second of the following series of quantities be all positive

$$\begin{array}{llll} a & b & c & \dots \\ \alpha & \beta & \gamma & \dots \end{array}$$

then the fraction

$$\frac{a+b+c+\dots}{\alpha+\beta+\gamma+\dots}$$

must lie between the greatest and least (algebraically speaking) of the following fractions,

$$\frac{a}{\alpha},\ \frac{b}{\beta},\ \frac{c}{\gamma},\ \&c.$$

Theorem 2. If S and Σ be given quantities, and if a, a', a'',

&c. approximate to the limit (a); α, α', α'', &c. to the limit (α); and so on: and if

$$\frac{\mathrm{S}}{\Sigma} = \frac{a+b+c+\ldots}{\alpha+\beta+\gamma+\ldots}$$
$$= \frac{a'+b'+c'+\ldots}{\alpha'+\beta'+\gamma'+\ldots}$$
$$= \frac{a''+b''+c''+\ldots}{\alpha''+\beta''+\gamma''+\ldots}$$
$$\ldots\ldots\ldots\ldots\ldots$$
$$\ldots\ldots\ldots\ldots\ldots$$

then $\frac{\mathrm{S}}{\Sigma}$ must lie between the greatest and least of the following fractions, if every term of all the preceding denominators be positive:

$$\frac{(a)}{(\alpha)}, \ \frac{(b)}{(\beta)}, \ \frac{(c)}{(\gamma)}, \text{ \&c.}$$

Theorem 3. If p and q be two quantities which have the limit a, then the limits of

$$\frac{\phi p - \phi q}{p-q} \text{ and } \frac{\phi(a+h)-\phi a}{h}$$

must be the same, in whatever manner p and q are supposed to vary, provided the limits be severally equal to a.

Theorem 4. If ϕx be a function which does not become infinite for any value of x between a and b, then

$$\frac{\phi(x+h)-\phi x}{h}$$

must have a finite limit, when h is diminished without limit, for *some* values of x lying between a and b.

Theorem 5. If any value of x be assigned, the differential coefficient of any function of x is finite, either for that value of x, or for one within *any* given degree of nearness to it.

Theorem 6. If the differential coefficient of ϕx be finite for every value of x between a and b, then $\frac{\phi a - \phi b}{a-b}$ must lie between the greatest and least value of the differential coefficients between $\phi' a$ and $\phi' b$.

In what follows, I shall use the abbreviation $a+\theta h$ for a quantity lying between a and $a+h$, both inclusive; that is, θ lies between 0 and 1, both inclusive.

Theorem 7. If $\phi'(a+\theta h)$ be always positive, $\phi(a+h)$ is

greater than ϕa; and if $\phi'(a+\theta h)$ be always negative, $\phi(a+h)$ is less than ϕa.

Theorem 8. If $\phi a = 0$ and $\psi a = 0$, and if $\psi'(a+\theta h)$ be always positive, then $\dfrac{\phi(a+h)}{\psi(a+h)}$ must be equal to some value of $\dfrac{\phi'(a+\theta h)}{\psi'(a+\theta h)}$.

Theorem 9. If ϕx and ψx be two functions of x which satisfy the following conditions:

$$\phi a = 0 \quad \phi' a = 0 \quad \phi'' a = 0 \ldots\ldots \phi^{(n)} a = 0$$
$$\psi a = 0 \quad \psi' a = 0 \quad \psi'' a = 0 \ldots\ldots \psi^{(n)} a = 0;$$

and if

$$\psi(a+\theta h) \quad \psi'(a+\theta h) \ldots\ldots \psi^{(n+1)}(a+\theta h)$$

be severally always positive when h is positive; then

$\dfrac{\phi(a+h)}{\psi(a+h)}$ must be equal to some value of $\dfrac{\phi^{(n+1)}(a+\theta h)}{\psi^{(n+1)}(a+\theta h)}$.

The preceding conditions are satisfied by

$$\phi x = f x - f a - (x-a) f' a - \overline{x-a}^2 \frac{f'' a}{2}$$
$$- (x-a)^3 \frac{f''' a}{2.3} - \ldots - (x-a)^n \frac{f^{(n)} a}{2.3\ldots n}$$

$\psi x = (x-a)^{n+1}$,
and we then have

$$\frac{\phi(a+h)}{\psi(a+h)} = \frac{f^{(n+1)}(a+\theta h)}{2.3\ldots(n+1)},$$

or

$$f(a+h) = f a + f' a . h + \ldots + f^{(n)} a \frac{h^2}{2.3\ldots n}$$
$$+ f^{(n+1)}(a+\theta h) \frac{h^{n+1}}{2.3\ldots n+1},$$

the only condition being that fa, $f'a$, ...$f^{(n)} a$ must not be either of them infinite.

This is the form in which Lagrange exhibited the theorem, and it never *fails*, as it is customary to say when a theorem which has been exhibited in too general a form shows symptoms of discontent at being asked to perform all that its godfathers promised for it.

I believe that any one may establish the preceding series of

propositions without reference to anything but the most common propositions of the theory of limits. Or at any rate, I feel certain that all who would be able to specify an objection will easily discover what are the processes which I consider as proofs. I therefore do not think it necessary to give the proofs, and my only object in writing this letter has been to call attention to the possibility of establishing this celebrated theorem by methods which certainly do not lie open to any objections arising from assumptions relative to expansion of functions.

I remain, Gentlemen, yours, &c.

London, July 20, 1835. ——— — ———.

XXIII. *On the Ancient and Modern Formation of Delta in the Persian Gulf by the Euphrates and Tigris, in answer to* Mr. Beke. *By* W. G. Carter, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

AFTER so long an interval, and when the points in discussion between Mr. Beke and myself must be nearly forgotten, I reluctantly enter on it anew. The manner, however, in which that gentleman has met and has construed my remarks in your October Number renders some notice unavoidable.

I quite concur in leaving the question,—what was the gopher wood of the ark?—where that discussion has placed it, and in so doing shall not superadd a remark to awaken it.

The topic more likely, perhaps, to engage attention at this moment, thrown into that inquiry, relates to the supposed extraordinary power of the Euphrates, Tigris, and neighbouring streams to form new land and expel the ocean, Mr. Beke still adhering to the theory which carries the Persian Gulf, at the date of the building of Babel, or about 100 years after the Flood, to the site of Babylon and beyond it to nearly one third of that extent. Col. Chesney makes the distance from the gulf to Hillah on this spot 451 miles*, so that the amount of land formed by the deposits of the rivers since about A.M. 2452 is on this hypothesis of no less length to travel over than 580 miles, or, in a line (adopting the $\frac{3}{4}$), 435.

Seeing, however, that Pliny†, the great authority for this theory, informs us in a passage to which we are referred that much earlier than his day (A.D. 78) a junction, which we now

* Report of Committee of House of Commons on Steam Navigation to India.

† Pliny, *Hist. Nat.*, l. vi. c. 27.

see, had even then taken place between the Euphrates and the Tigris, and that a long space then also existed between that junction and the sea, the first glance at this author seemed to me rather to give a direct negative to the whole matter.

But going back 400 years before Pliny, to the reign of Alexander the Great, I adduced in my last paper the voyage of Nearchus, illustrated by the masterly exposition of that subject by Dean Vincent, and the navigation of those rivers performed by Alexander and his fleet, as affording conclusive facts in proof that the broad outline of the coast had, as to any point in question between Mr. Beke and me, undergone no material change for the last 2200 years. This is all passed over in the reply; but it states that I had "asserted the fact," "much strained and qualified many ancient authorities," and then, from some early measurements, in all their uncertainty, which Pliny, while he gives them, points out, it concludes that "a very considerable advance of land" (the great delta, I presume,) "is a fact established beyond dispute." Not quite so, I apprehend. Let us examine a little.

A delta which has been forming during 3300 years, and whose formation is said to have been in high activity at the end of 1600, or the commencement of the Christian æra, can offer no territory, in the whole or nearly the whole of any line of its extent, which was the scene of very early events. Take Susiana, being that part of it on the east or north-east of the Tigris. Its capital, Susa, is said to have been founded by Tithonus*, father of the celebrated Memnon; its origin, in fact, lost in remote antiquity, but pointing to about 1200 years B.C., at less than a ninth of this delta's present age. Nebuchadnezzar, about 600 B.C., had a palace at Shushan (probably Shuster), on the Susian Ulai or Euleus. Susiana was one of the satrapies of Darius Hystaspes, 500 B.C.† Turning to this delta's longer line, let us take the Babylon of Nebuchadnezzar. At that time the city being about the site of Hillah, there could have been, on this theory, little or no advance of the delta beyond it. We have now, at the lowest calculation, on the common chronology, less than a thousand years to the division of the lands, and yet in the time of Isaiah, more remote still by nearly two hundred years, the identical Chaldean Babel is spoken of as a settlement of the earliest antiquity. "Behold," says Isaiah, "the land of the Chaldeans; this people was not, till the Assyrian founded it for them that dwell in the wilderness; they set up the towers thereof, they raised up

* Strabo, l. xv. 729. † Herodotus, *Thalia*, 91.

the palaces thereof*." No one can doubt that this refers to the celebrated Babylon. We are here, then, close upon Peleg, with a settlement and colony at the end of a delta, intruding 135 miles on a site which the supposed gulf of his day covered.

I must now return to the country of Susiana, further to show by its ancient limits, and the navigation of Alexander the Great and his officers, how little ground there is for the addition to the delta of the 245 (451 — 206) miles,—too little, as it is, for the theory,—said to have been made since that period.

The remarkable river Arosis, or Oroatis, now called the Endian, and in the country, Tab †, or 'the river', by way of preeminence, following on from the coast of the delta, formed with the gulf, and identifies, its south or south-eastern boundary. Thus Ptolemy‡ makes that boundary, the gulf from this river to the Tigris. Nearchus, we shall find, says the same. Strabo§ mentions the Oroatis as the Persian boundary, and adds that all the rivers which flow through Susiana fall into the channel of the Tigris and the intermediate canals of the Euphrates. Here we have, too, as in Pliny, distinctly the junction of the rivers, a considerable space between it and the sea, and the united stream, called by Strabo the Euphrates, thus corresponding to the state of the country at this day.

Now, let us take the navigation of Nearchus and his fleet from the mouths of this river Arosis (325 B.C.). In going thence along the coast of Susiana, he tells us ‖ "that the place was very marshy, and that the shoal extended far into the sea....They came to anchor at Kataderbis....Thence sailing away, the ships were led one by one through narrow channels; stakes fixed on both sides pointed them out." Thus they proceeded, and the next day from Kataderbis came to Diridotis, on the south-west point of the delta; so that instead of having a voyage of ten or eleven days, which the present theory would give them, they arrived from the boundary stream of Susiana on the opposite side, by a very difficult and dangerous course, on the third day from starting. On this topic the reply is silent, and it is the more remarkable because Mr. Beke says, "in reply to Mr. Carter, I ought, perhaps, to confine myself to the consideration of Nearchus's statement alone."

* Is., ch. xxiii. v. 13.

† Vincent's Commerce and Navigation of the Ancients, vol. i. p. 418.

‡ Ptolemy, 149. § Strabo, l. xv. 728.

‖ *Την χωρην τε τεναγωδεα τε ειναι την πολλην και ῥηχιησιν επι μεγα ες τον ποντον.... Ορμιζονται επι Καταδερβις....Ευθενδε δε ὑπο την ἑω εκπλωσαντες κατα βραχεα εκομιζοντο επι νεως. Πασσαλοισι δε ενθεν και ενθεν πεπηγοσιν απεδηλουτο τα βραχεα.—Arrian's Nearchus*, 40 and 41.

Then as to the navigation of Alexander and his fleet in the delta streams. Arrian thus gives it *: Alexander, having joined his fleet at Susiana, "sailed down the river Euleus towards the sea; and now when near its outlet to the ocean, he left the worst and greater part of his ships, and with the swiftest sailed down the Euleus to the ocean and to the mouths of the Tigris, the other ships on the Euleus entering a canal cut from the Tigris to that river." He thus made a circuit to meet them. And all this might be done at the present time. The ancient canal, the entire circuit, all the points of the navigation then presented by the spot, are still offered for our observation. The Euleus, coming from the north-east in a line perpendicular to that of the Tigris, actually *marks off* and fixes the extent of the delta of that period. Or are we, for the purposes of this theory, to suppose some other river? flowing too through Susiana, which lies in part at the end of a line from the lower Euphrates†, carrying its waters to a delta 200 or 300 miles to the north-west, and another canal cut across from it to some high quarter of the Tigris, adapting itself to the occasion? But that is impossible; the theory throws an expanse of gulf in its way, into which any river from Susiana which could be substituted must have fallen, remote from the Tigris.

On my "much straining and qualifying many ancient authorities," I do not think it necessary to enter, especially as they all harmonize with the *unbroken* sense of the passage in Pliny, in part quoted by Mr. Beke, and to which I shall presently refer. My observation, distinctly applied to Pliny's general account of the two rivers only, that gentleman at once transfers to all the authorities adduced, and then adds, "on his own admission they are not always very explicable‡."

But the main point on which Mr. Beke now rests his argument is the measurement of distances given by Pliny. The quotation from that author which seemed the most to favour

* Κατεπλει κατα τον Ευλαιον ποταμον ὡς επι θαλασσαν. Ηδη δε πλησιον ὡν της εκβολης της ες τον ποντον τας μεν πλειονας τε και πεπονηκυιας των νεων καταλειπει αυτου αυτος δε ταις μαλιστα ταχυναυτουσαις παρεπλει απο του Ευλαιου ποταμου κατα την θαλασσαν ὡς επι τας εκβολας του Τιγρητος. Αἱ δε αλλαι αυτω νηες ανακομισθεισαι κατα τον Ευλαιον ες τε επι την διωρυχα ἡ τετμηται εκ του Τιγρητος ες τον Ευλαιον.—*Arrian, Exp. Alex.*, l. vii. c. 7.

† See Col. Chesney's Map in Report.

‡ A few miles of addition to the delta has manifestly never been the question. I much object to such expressions in the reply as, "Mr. Carter has, in fact, asserted the opinion, that since the time of Nearchus, the encroachments on the gulf must be very unimportant," omitting the words "*to the point in question*, any later encroachments," &c., as conveying the idea of a mere assertion without proof, and a much broader one than my remarks warrant.

it was, that in ancient times a space of several miles existed between the mouths of the two rivers, because it led us to infer that all this space had been since filled up by the growth of the delta. In my last paper I pointed out a serious oversight; for Pliny immediately goes on to say, "but long since the Orcheni and neighbouring people had dammed up the Euphrates to water the fields, so that by the Pasitigris only it is now carried to the sea *." I should not here notice the important loss to the theory of this passage, but that the reply does not, and a similar instance occurs.

I had there made a pointed reference to the following statement of Pliny †: "Nearchus and Onesicritus relate, that the length of the voyage along the Euphrates, from the Persian sea to Babylon, is 412 miles;" (then follows the distance from Seleucia;) then, "Juba says, from Babylon to Charax is 175 miles....Some say that beyond Babylon it keeps in one channel for 87 miles, till parted at the irrigating canals....From such varying relations it is difficult to learn the true measure....Where it ceases to keep its channel on the confines of Charax," &c. So that we have for the distance in miles to Babylon 412, with Juba's 50 to Charax also 225, and a third in this 87 only to about Charax. The length of the stadium is not the point here. Indeed, having examined several distances in Strabo and Pliny, I believe 16 to a mile, notwithstanding their rule (*infra*) will *sometimes* correspond fully as well as any other. *The point* is, that the distance was *utterly uncertain*. Pliny knew not, could not know, what to determine, and consequently whatever he has related of an increase in the distance of Charax from the sea, and thus of some large accessions to the land, must rest on the slippery basis of this "diversitas auctorum".

The reply, however, having in the preceding paragraph introduced Dean Vincent to say that Nearchus not having personally made the estimate, it could not be relied on, detaches it from the rest, and thus presents it: "We fortunately possess an authority independently of Arrian ‡, who establishes

* Pliny, *Hist. Nat.*, l. vi. c. 27. And it is the more remarkable, for Pliny actually mentions this junction of the Euphrates and Tigris no less than *three* times in the same sixth book, and notices there also, specially, the confluence of the Tigris and Euleus.

† Euphrate navigari Babylonem e Persico mari 412 mill. pass. tradunt Nearchus et Onesicritus....Juba a Babylone Characem 175 mill. pass.... Fluere aliqui ultra Babylonem continuo alveo priusquam distrahitur ad rigua 87 mill...Inconstantiam mensuræ diversitas auctorum facit...Ubi desinit alveo munire ad confinium Characis, &c.—*Pliny*, *Hist. Nat.*, l. vi. c. 26.

‡ Pliny had learned this from some quarter, but why is he to be considered an authority independently of Arrian for the 3300 stadia? He ex-

that historian's correctness upon this subject in all points. This authority is Pliny, or rather Juba as cited by Pliny, who states, 'Euphrate navigari Babylonem e Persico mari 412 mill. pass. tradunt Nearchus et Onesicritus.'" Who could suppose there was such a sequel? Yet this insulated notice, labouring under all the uncertainty as to Nearchus's correctness and Pliny's alleged error and avowed difficulty, with his account of discordant measurements in the first part of the same river voyage, as far as Charax, are the important points in support of the theory; the main difference between the whole and the part appearing to be, that the number of miles in the one is from the higher to the lower, in the other from the lower to the higher; but as both seem to give them in the order of time in which they were taken, it is unaccountable that Pliny should hence have inferred, as he seems to have done, that the differences in the first part of the voyage,—differences, too, not corresponding with those of the whole distance, and from which the confusion he complains of had chiefly arisen,—should lead him to reconcile the former only by the notion that a large increase had therefore been made to the land; and certainly serious doubts may thus well arise of the authenticity of the latter passage.

The reply, to remove a difficulty about the distance of Charax from the sea, would suppose "the more correct construction" to be, that this city was built by Alexander 50 miles inland, Pliny having stated it in his time to be 120, thus making the whole advance of the delta, from Alexander to Pliny, 70 miles only. And we have seen, that long before the latter, the two rivers met and passed in one channel to the sea. Now, we shall not, I presume, be required to believe, that not only an addition of some 200 miles of land has been made to this delta since Pliny wrote, but also that the rivers have separated, formed each a channel over it, and then again formed a junction and another joint channel, in due correspondence with

pressly says, "Priusquam hæc generatim persequamur indicare convenit quæ prodit *Onesicritus* classe Alexandri circumvectus in Mediterranea Persedis ex India narrata proxime a Juba;" and he tells us, "Onesicriti et Nearchi navigatio nec nomina habet mansionum nec spatia." Yet, as Salmasius observes (830), Nearchus's account relates both very fully, and "Plinius ex auctore Onesicriti eamdem retulit mirum quanta diversitate." Had Pliny or Juba known much of Arrian's Nearchus, would they have followed the loose story of Onesicritus? There is an authority, however, for about 3000 stadia, and it is Strabo; but we know that Strabo's *rule* was the stadium of 8 to a mile. He makes the distance from Thapsacus (El Deir) to Babylon 4800 stadia; adding the 3300 thence to the sea, we get 8100, which, at 8 to a mile, is 1012 miles: Col. Chesney makes it 1025 miles (Strabo, xv. 146. Report, &c.), a difference between the ancient and modern admeasurements of 13 miles only. If measurements are to decide on the growth of the delta, what has been offered so conclusive as this?

his account of them. This surely will not be supposed. But unless it be, what becomes of all that is said there, and before and after this, in the next sentence, is repeated, about Pliny's error? The 3300 stadia which Alexander's officers had reckoned it from the sea to Babylon, here at least, could not possibly have been the stadia of 16 to a mile, or 206½ miles only. If the distance of Charax, the port, had increased but 70, the whole distance to Babylon could have increased but 70; and then "it is yet further to be considered" whether even this 70 is not to be reduced, in fact, to 35 only; so that without the above extravagant hypothesis, the great point of 245 miles more of delta since the voyage of Nearchus (B.C. 325) is thus at once disposed of*.

It is due to Dean Vincent (whom the reply unceremoniously throws overboard†) to say, that no one felt the difficulty of adapting a fixed standard of ancient measurement to his subject more than he did. In employing, with M. D'Anville, the so-called stadium of Aristotle, he admitted that it is not to be found in Aristotle; that it is not, "perhaps, possible to measure 500 stadia in any detached portion of the [Nearchus's] course with satisfaction;" that the stadium of the generality of ancient writers is eight to a Roman mile; and that D'Anville's measures "still leave some obscurity behind‡." Major Rennell insists on a much longer one than Aristotle's§. Pliny|| and Strabo¶, who both mention Nearchus's voyage and the distance to Babylon, both notice the stadium as being that of eight to a mile, and Dr. Falconer, after a learned investigation, concludes this to be the stadium of Arrian and the other earlier writers. Surely the length of the stadium offers no sound basis for a theory**.

But Mr. Beke says I have not touched upon the geological

* And without supposing all this, even the increase of 35 is annihilated by the plain fact, that it is now but 62 miles even to Bosra. The junction of the Susian river is about 20 miles lower down (see Col. Chesney's map in Report). We are to place Charax 5 miles still lower, and even halving the 120 of the merchants and Arabs, (on which the inference is made to rest,) we have then 60 to the sea, while at the present moment, from the same spot, it is thus but about 35 miles only.

† Malte-Brun seems to have been quite as nearsighted on the general topic as the learned Dean, for he observes of the mighty Indus and its far-spreading tributaries, "From the voyage of Nearchus we should learn, that notwithstanding the immense tides, the coasts, at the mouth of the Indus, have not been sensibly changed since the time of Alexander." (Universal Geography, vol. i. book 18.)

‡ Vincent's Commerce, &c., Prelim. Dissert., 9.

§ Geography of Herodotus, sect. 2.

|| Pliny, *Hist. Nat.*, l. ii. c. 23.

¶ Strabo, l. vii. 322.

** Dr. Falconer's Arrian's Voyage, Discourses, &c., p. 184.

portion of the argument. I have not found it. The mere fact that rivers form deltas and produce more or less changes on the surface, and that these rivers have done so, no one disputes. I believe this delta's existence is now disproved by history. But a word or two on the desired topic.

There seems no other doubt than that in discussion, that all below Felujah, or thereabouts, in the course of the Euphrates, has been, at some period, *geologically* modern, a part of the desert, and, like all sandy deserts, once covered by the ocean: that accounts for its level character. And, from the tendency which the waters of an inlet of the ocean, like that supposed, have to rise, as have long been known in the instance of the Red Sea, I fear, paying no respect even to the wide boundaries now given to it, it must necessarily have travelled over all the Arabian desert. What was to stay it? The gulf current drives that way. The protecting slip of country was not yet raised by the ordinary deposits of the river. The measure of this delta is clearly the extent of the level. That slip is no true exception. On the principle adopted, all the desert has an equal claim. But in forming deltas, rivers do not construct sandy deserts; so that is excluded.

In the opening made by Alexander to the Pallocopas, in order to limit the efflux of water, and therefore, probably, at no great depth, the workmen appear to have come to rock within about 30 stadia of the Euphrates, ὑποπετρος ἡ γη εφαινεται. From the context this was plainly not mere gravel,—a very unlikely discovery many miles laterally within, and some 200 from the head of, a delta *.

A river, in producing a delta, forms both its own bed and channel necessarily, at first, in the proximate line of its course. If it can thus obtain a sufficient slope, (and where does it?) the channel thus formed will also necessarily be nearly straight; but as the inclination which its delta acquires, in either a direct or transverse line, must be very moderate, the stream naturally divides into several channels. Both these rivers, however, and especially the Tigris, instead of pursuing a direct course, are here tortuous and serpentine, and, for the most part, keep each within a single channel. The Tigris passes over the alleged delta in a rapid stream, consequently finds a considerable slope; the Euphrates from above Felujah, for upwards of 600 miles, in a dull and lingering stream, consequently with scarcely any, and to its low level the Tigris, " very different in every respect †", at length bends its course and joins it at

* Arrian, *Exped. Alex.*, l. vii. c. 21.

† Col. Chesney's Evidence and Remarks in Report.

Khorna*. From Khorna, then, there must be an ample rise north-west along the course of the Tigris, and from it a descent south-west towards the level of the Euphrates; and as the rivers of Susiana run in this direction †, there must be a further rise over the delta through Susiana to the mountains. Whilst, on the other hand, the current of the gulf setting strongly from the north-east to the south-west,—from the direction of the Tigris towards that of the Euphrates,—must, on this theory, rather have raised the relative level of the Euphrates' channel throughout the mighty embankment.

But we see the rise is, in fact, the other way, and it is manifestly impossible to reconcile the degree and extent of this variation of level and the general phænomena of the supposed delta, with the existence here of such a formation. Indeed, were it so, the Tigris, in thus constructing its own bed and channel, must *necessarily have done it in the direction of the declivity towards the Euphrates*; and thus, at the earliest stage of this progress, would *inevitably have fallen into the channel of that river.* We should then have had, for a short space, the united stream in one channel, which would doubtless, as in other instances, and as these waters do below Bosra, have spread out into diverging streams till they reached the gulf.

And, now, on this topic, to revert to the point we started from, the gulf being 580 miles up the country 100 years after the flood. Even supposing for a moment all that extent of land to be the production of this agency, it is geologically a little singular that the inquiry did not arise, What connexion has that event with the topic? It has before been admitted, that it

* Both at a very early and in the present day clouds have hung about this topic. Arrian, l. vii. c. 7., says expressly, the Euphrates has a higher channel than the Tigris, which receives the waters of the Euphrates by many streams. Prof. Heeren (*De la Politique, &c. de l'Antiquité*, Fr. ed., vol. ii. s. 2.) refers to, and seems to have been misled by, this passage, for though he quotes from the proceedings of the Bombay Society, "the tide passes up the Euphrates for 20 miles above Khorna, and stops there at the mouth of the Tigris," (Mr. Barker in Evidence, Report, &c., says there is no perceptible tide at Bosra, 12 miles below Khorna,) to show that the Tigris is far the more rapid river, he overlooks the fact that it can be more rapid only through flowing from a higher country down a greater slope. Xenophon understood this better: he mentions four canals by which the latter pours its waters into the Euphrates (*Anab.*, l. i. c. 7.). The same may be inferred from Justin (42. 3.); and Captain Chesney (Report, &c.) informs us of the canal of "the Hie by which the Tigris gives a large contribution to the sister stream," about 220 miles above the gulf. The error may have arisen from the Euphrates having raised its bed above the immediate level over which it passes. Its waters, therefore, would run from it through the irrigating canals till they came to the rise towards the Tigris.

† Strabo, l. xv. 728.

changed neither the character nor the products of this country. Did not its waters come hither and return as in other places it might reach? Might not these rivers have run, and their joint delta been formed, thousands of years before the cataclysm? Geology knows of none other than *relative* dates. Thus, were even the delta established, we should yet be no nearer to the proof that the gulf was in its site in the days of Peleg.

Mr. Beke, with much candour, relinquishes the reading of Gen., x. 11, that Asshur was the person who built Nineveh, and adopts the opinion that Asshur (or Assyria) was the country and Nimrod the builder; though it appears from the sequel that he still adheres to a second Babel in the Babylon of the Chaldeans. But as Shinar must have embraced no very extensive range,—for its little chief and three others Abraham and his servants vanquished,—and Nimrod's Babel (or Babylon) and the Babylon of the prophets were in it, that we have here two places of the same name,—both moreover, as we have seen, built in the very infancy of society, and both by "the Assyrian", is, I apprehend, an inference not sanctioned by true historical construction.

The opinion, however, is repeated, of the improbability, at all events, of cities having been built (or, more properly, of settlements having been made), at the earliest post-diluvian age, in the low lands of the Euphrates. No allusion is made to my answer that the cities and settlements of a hot climate, and more particularly those of an early people, are of necessity, and that the earliest on record were, in fact, fixed in such places. The travellers from or towards the east settled in "a plain", and any plain in which they could then have settled becomes immediately liable to all the objections made to that spot, in which the history, according to every authority, I believe, (save Drusius,) says that they did settle. Col. Chesney's evidence*, relieves us from further discussion. He tells us that along the Euphrates "it is no uncommon occurrence to see a whole village afloat, and the people following it on foot or in their canoes to arrest the materials of their dwellings." In a cold climate we have little notion of the value of water to the inhabitants of a hot one. Here, however, we have the Nimrods of the present day, in spite of the experience of nearly 4000 years, doing just what their own and the other earliest patriarchs did before them, who, heedless of all the good reasons to the contrary, seem to have chosen for their little settlements every such impracticable spot they could find. They built Nineveh

* Report of Committee of House of Commons, &c.

"of old like a pool of water," Calneh, Babylon, &c. "in the low lands of the Tigris and Euphrates"; they resorted to the valley of the Nile, fearless of the "flood of Egypt"; and they peopled the vale of Siddim in the plains of the Jordan, "which overflowed all its banks in the time of harvest".

[To be continued.]

XXIV. *On certain Transformations connected with the finite Solution of Equations of the Fifth Degree.* *By* G. B. Jerrard, *A.B.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

SOON after I had discovered, by the method of investigation contained in Part ii. of my "Mathematical Researches", that the second and third terms of the transformed equation in y might be made equal to zero, independently of one of the indeterminate quantities, Q, involved in the expression for y, and that consequently the general equation of the fifth degree might be reduced to any one of the trinomial equations

$$x^5 + \mathrm{D}x + \mathrm{E} = 0,$$
$$x^5 + \mathrm{C}x^2 + \mathrm{E} = 0,$$
$$x^5 + \mathrm{B}x^3 + \mathrm{E} = 0,$$
$$x^5 + \mathrm{A}x^4 + \mathrm{E} = 0,$$

I perceived, on comparing these forms with the solvable ones which were known to mathematicians, that had it been proposed to arrive by a direct process at the third of these equations,

$$x^5 + \mathrm{B}x^3 + \mathrm{E} = 0,$$

and at De Moivre's solvable form,

$$x^5 + \mathrm{B}x^3 + \tfrac{1}{5}\mathrm{B}^2 x + \mathrm{E} = 0,$$

the same analytical difficulty arising from the dimensions of the equations of condition must in both instances have been overcome; since in the former case it would have been necessary to make

$$\mathrm{A} = 0,\ \mathrm{C} = 0,\ \mathrm{D} = 0,$$

and in the latter,

$$\mathrm{A} = 0,\ \mathrm{C} = 0,\ \mathrm{D} - \tfrac{1}{5}\mathrm{B}^2 = 0.$$

This led me to suspect, notwithstanding the almost overwhelming weight of authority which pressed against the supposition, that there must be some defectiveness in the train of reasoning by which MM. Ruffini and Abel (in following out the views

of Lagrange) were believed to have *demonstrated* the impossibility of solving, generally, and in finite terms, equations of higher degrees than the fourth; an idea which has been amply borne out by the results at which I have subsequently arrived. I am, Gentlemen, yours, &c.,

West Park, Bristol, June 12, 1835. G. B. JERRARD.

XXV. *On* Olbers's *Method of determining the Orbits of Comets. By Professor* ENCKE.

[Continued from p. 132.]

IT only remains now to determine the elements of the orbit from the known quantities ρ, ρ'', r, r'', k. One might use all these five quantities and obtain convenient expressions for every single element. But here, likewise, the way recommended by Gauss, who only employs the first two quantities ρ and ρ'', seems to be more suited to the purpose. In this manner, without making the calculation much longer, the great advantage is gained of having a sure check on the accuracy of all preceding calculations, as the values thus found must exactly accord with the given data. The formulæ which Gauss proposes are, again, adapted to the use of his logarithmic tables, and exceedingly convenient in their application. I shall, however, add to some of them other formulæ, of which the *Theoria Motûs* presents such a number, which do not require the use of these tables, but seem for that very reason to be inferior in respect of the accuracy of the numerical results, on account of the uncertainty of the last figure of the logarithms.

Gauss designates

The heliocentric longitudes of the comet at the first and third observations by	λ λ''
The heliocentric latitudes by	β β''
The longitudes on the orbit by	v v''
The longitude of the ascending node by	☊
The inclination of the orbit, which, according to the usual distinction of the direct and retrograde motion of comets, is always less than 90°	i
The longitude of the perihelion	ω
The time of passage through the perihelion	T
The distance in the perihelion	q

The following calculations must be made in succession:

$$\rho = \frac{u + g\cos\phi}{h}$$

$$\rho'' = M\rho.$$

$$
\text{(III)}\quad
\begin{aligned}
&\rho\ \cos(\alpha-\Theta) - \mathrm{R} &&= r\cos\beta\ \cos(\lambda-\Theta)\\
&\rho\ \sin(\alpha-\Theta) &&= r\cos\beta\ \sin(\lambda-\Theta)\\
&\rho\ \tan\delta &&= r\sin\beta\\
&\rho''\cos(\alpha''-\Theta'') - \mathrm{R}'' &&= r''\cos\beta''\cos(\lambda''-\Theta'')\\
&\rho''\sin(\alpha''-\Theta'') &&= r''\cos\beta''\ \sin(\lambda''-\Theta'')\\
&\rho''\tan\delta'' &&= r''\sin\beta'',
\end{aligned}
$$

by which the heliocentric places are given. The accordance of the r and r'' with those obtained by the trials is the first proof of correct calculation. The comet is direct or retrograde, according as λ'' is greater or less than λ.

Next comes the determination of ☊ and i by these formulæ:

$$
\text{(IV)}\quad
\begin{aligned}
&\pm\tan\beta = \tan i\sin(\lambda - ☊)\\
&\pm\frac{\tan\beta'' - \tan\beta\cos(\lambda''-\lambda)}{\sin(\lambda''-\lambda)} = \tan i\cos(\lambda - ☊),
\end{aligned}
$$

where the upper signs refer to direct, the lower ones to retrograde, motion of the comets. Instead of these, the following formulæ may be applied:

$$\tan i\sin\left(\tfrac{1}{2}(\lambda+\lambda'') - ☊\right) = \frac{\pm\sin(\beta''+\beta)}{2\cos\frac{1}{2}(\lambda''-\lambda)}\sec\beta\sec\beta''$$

$$\tan i\cos\left(\tfrac{1}{2}(\lambda+\lambda'') - ☊\right) = \frac{\pm\sin(\beta''-\beta)}{2\sin\frac{1}{2}(\lambda''-\lambda)}\sec\beta\sec\beta''.$$

The longitude on the orbit is obtained by these formulæ:

$$
\text{(V)}\quad
\begin{aligned}
&\tan(v - ☊) = \frac{\tan(\lambda - ☊)}{\cos i}\\
&\tan(v'' - ☊) = \frac{\tan(\lambda'' - ☊)}{\cos i},
\end{aligned}
$$

where $v - ☊$ and $v'' - ☊$ must be taken in the same quadrants in which $\lambda - ☊$ and $\lambda'' - ☊$ are respectively situated.

If a further check on the calculation should be deemed necessary, one may calculate

$$k = \sqrt{\left\{\left(r'' - r\cos(v''-v)\right)^2 + r^2\sin(v''-v)^2\right\}},$$

where the value of k must perfectly agree with the former.

For the longitude of the perihelion and the distance we have these formulæ:

$$
\text{(VI)}\quad
\begin{aligned}
&\frac{1}{\sqrt{q}}\cdot\sin\tfrac{1}{2}(v-\omega) = \frac{\operatorname{cotan}\frac{1}{2}(v''-v)}{\sqrt{r}} - \frac{1}{\sin\frac{1}{2}(v''-v)\sqrt{r''}}\\
&\frac{1}{\sqrt{q}}\cdot\cos\tfrac{1}{2}(v-\omega) = \frac{1}{\sqrt{r}},
\end{aligned}
$$

in place of which one may also use these:

$$\tan(45^\circ + \omega') = \sqrt[4]{\left(\frac{r''}{r}\right)}$$

$$\frac{1}{\sqrt{q}} \sin F = \frac{\tan 2\,\omega'}{\sin \frac{1}{4}(v'' - v)\,\sqrt[4]{r\,r''}}$$

$$\frac{1}{\sqrt{q}} \cos F = \frac{\sec 2\,\omega'}{\cos \frac{1}{4}(v'' - v)\,\sqrt[4]{r\,r''}}$$

$$\omega = \tfrac{1}{2}(v'' + v) - 2\,F.$$

For the determination of ω', it will, however, be advisable to use logarithms of more than five places.

If we now enter with the true anomalies $v - \omega$, $v'' - \omega$, or $\omega - v$ and $\omega - v''$, into Barker's table, or any one of the other tables for the parabolic motion of comets, we shall find the time which has elapsed since the passage through the perihelion, or which will elapse till that moment; and the double determination of the time of passage will be the last proof of the correctness of the calculation.

For Barker's table we have, if M and M″ stand for the mean motions which correspond to the true anomalies,

$$\text{(VII)} \qquad T = t \mp M\,n\,q^{\frac{3}{2}} = t' \mp M''\,n\,q^{\frac{3}{2}},$$

where the constant $\log n = 0{\cdot}0398723$, and the upper signs are to be taken for a direct motion if $v > \omega$, $v'' > \omega$, or for a retrograde motion if $v < \omega$, $v'' < \omega$; and the lower signs for the contrary cases.

In order to be perfectly satisfied of the correctness of the calculation, it will now be advisable to calculate the place of the comet at the time of the second observation, in order to compare it with the one actually obtained by observation. By the nature of the proceeding, one will easily perceive that this middle observation was only used for calculating M, and even there only one new quantity (the angle Σ') was employed, derived from the two data of the observation, as the distance σ' disappears from the expressions both of M′ and M″. The rigorously conducted calculation will, therefore, even when the complete value of ρ'' is applied, give accurately only this angle, or the quantity

$$\tan \Sigma' = \frac{\sin(\alpha' - \Theta')}{\tan \delta'},$$

and the effect of the errors on the middle longitude and latitude will consequently be thus:

$$d\,\alpha' = \frac{\sin \Sigma'}{\cos \delta^{2}} \,.\, d\,\sigma'$$

$$d\,\delta' = \cos(\alpha' - \Theta')\cos \Sigma' \,.\, d\,\sigma',$$

or the ratio of these errors

$$\frac{\cos \delta' \,.\, d\,\alpha'}{d\,\delta'} = \frac{\tan(\alpha' - \Theta')}{\sin \delta'}.$$

If the orbit of the comet accords so nearly with the observations that $d\,\sigma'$ is in itself very small, the magnitude of this ratio need not further be taken into consideration. But if from some cause or other $d\,\sigma'$ is not insensible, and if the greatness of the ratio indicates a very unequal distribution of the errors, then we might have a reasonable inducement to correct the orbit on a principle different from that which is the foundation of Olbers's method: this may generally be most easily effected by two or three hypotheses for ρ or $\frac{\rho''}{\rho}$. The principle of Olbers may, agreeably to Bessel, be most simply expressed in this way: *that the orbit, while it strictly passes through the two extreme places, likewise accords with the great circle passing through the middle places of the sun and comet.*

The exact agreement of the Σ' obtained by the last results of the calculation with the one involved in the observations, may always be attained by Olbers's method; but this can only be done with perfect accuracy if the rigorous expression of (18) or the correction of (26) and (27) is applied: and, inversely, this agreement, if perhaps attained by the approximate value of M only, proves that the rigorous expression would not much differ from the approximate one. It affords a proof that all further correction would be superfluous. If, however, the calculated value of Σ' should not be found to agree with the other, without any error of calculation having crept in, Carlini has proposed, if the difference is so small as to effect only the last decimals of tan Σ', to use an artifice similar to the one explained by Gauss in his summary view of the calculation of the orbits of planets. Without applying the strict correction, let a new value of M be calculated by means of a value of Σ' which deviates from the one derived from the observations by an equal quantity, but in a contrary direction, as the one found from the first calculation of the orbit, and the new orbit thence deduced will give the value deduced from the observations accurately, or at least very nearly so.

[To be continued.]

XXVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

1835. June 4.—THE reading of a paper, entitled, "On the Influence of the Tricuspid Valve of the Heart on the Circulation of the Blood." By T. W. King, Esq., M.R.C.S. Communicated by Thomas Bell, Esq., F.R.S.,—was resumed and concluded.

The purport of this paper is to prove experimentally that the tricuspid valve of the human heart does not, in the ordinary state of the circulation, completely prevent the reflux of blood from the ventricle into the auricle on the right side, and that the amount of regurgitation is continually varying according to the different degrees of distention of the ventricle. The author points out the anatomical differences between the auriculo-ventricular valves on the right and left sides of the heart; from the consideration of which it might have been inferred, independently of direct experiment, that while the structure of the mitral valve is adapted to close accurately all communication between the left auricle and ventricle during the contraction of the latter, that of the tricuspid valve is designedly calculated to allow, when closed, of the flow of a certain quantity of blood from the right ventricle back again into the auricle. The comparatively imperfect valvular function of these latter membranes is shown by various experiments on recent hearts, in which it was found that while fluids injected into the left ventricle, through the aorta, were perfectly retained in that cavity, by the closing of the mitral valve, when the right ventricle was similarly injected through the pulmonary artery, the tricuspid valves generally allowed of the escape of the fluid, in streams, more or less copious, in consequence of the incomplete opposition of their margins. On repeating these experiments on different animals the author obtained similar results; but found that the imperfection of the valvular function was greater the sooner the heart was examined after the death of the animal; and that if the trials were made after the lapse of a certain time, the rigidity which gradually supervened on the muscular fibres of the heart, and of the carneæ columnæ attached to the margins of the valves, brought them into more complete apposition and led to the accurate closing of the passage. This effect, however, was never so perfectly accomplished in the tricuspid as in the mitral valves.

The author regards this peculiarity of structure in the tricuspid valve as an express provision against the mischiefs that might result from an excessive afflux of blood to the lungs, operating as a safety-valve against accidents of a serious character, and as more especially advantageous in incipient diseased enlargements of the right ventricle. He adverts to the conditions of the heart during the fœtal state of existence, in which the same necessity of guarding against excessive pressure does not occur, and where the structures are found to correspond to the variation of functions. A similar adjustment of the right auriculo-ventricular valve to the peculiar circumstances and habits of animals may also be traced by extending the inquiry to various classes of animals.

"Report of a Committee for collecting Information respecting the occurrence of, and the more remarkable Phænomena connected with, the Earthquakes lately felt in the Neighbourhood of Chichester." By J. P. Gruggen, Esq. Communicated in a letter to P. M. Roget, M.D., Sec. R.S.

This paper contains an authentic report of several shocks of earthquakes which, during the last two years, have been felt at Chichester and the surrounding country; drawn up from accounts given by various correspondents, in answer to printed queries extensively circulated. The first shock occurred on the 18th of September, and the second on the 13th of November, 1833. Another and more severe shock was felt on the 23d of January, 1834, and in the latter end of the same year two slighter shocks were experienced, namely, one on the 27th of August, and the next on the 21st of September; the last, which was less than any of the former, took place on the 12th of January, 1835.

The Society adjourned over Whitsun week to meet again on the 18th instant.

June 18.—The following papers were read:

"Discussion of Tide-Observations made at Liverpool." By J. W. Lubbock, Esq., V.P. and Treas. R.S.

The author has here presented to the Society, by permission of the *British Association for the Advancement of Science*, a discussion by M. Dessiou of about 14,000 tide-observations made at Liverpool, on the plan similar to that adopted with regard to the London Dock observations. The first book contains the moon's transits, classified with the moon's parallax and declination, together with the date and corresponding time and height of high water; the height of the barometer is also added to the observations of about four years. The second book contains the same quantities, classified further according to the different calendar months, and for each minute of the moon's horizontal parallax. The third book contains a similar classification for the moon's declination. The average results are given in tables at the end.

Some remarks are subjoined on the registers of the observations taken at the London and St. Katherine's Docks; from which it appears that the tide is about five minutes earlier in the former than in the latter of these two places; and that the difference in height is about five feet.

"On the Star-fish of the genus *Comatula*, demonstrative of the *Pentacrinus Europæus* being the young of our indigenous Species." By John V. Thompson, Esq., F.L.S., Deputy Inspector General of Hospitals. Communicated by Sir James Macgrigor, Bart., F.R.S.

The author states that the *Pentacrinus Europæus*, which is fixed by its stem to other bodies, and consequently deprived of the power of locomotion, is produced from the ova of the *Comatula*, and becomes in a subsequent stage of its evolution detached, assuming the form of this genus of *Asterida*, and capable of moving freely in the ocean; at one time crawling amongst submarine plants, at others floating to and fro, or swimming in a manner similar to *Medusæ*.

"On the Ova of Women and Mammiferous Animals, as they exist in the Ovaries before Impregnation." By Thomas Wharton Jones, Esq. Communicated by Robert Lee, M.D., F.R.S.

After reviewing the accounts given by various authors of the structure of the ovaries, corpora lutea, and ova in different tribes of animals, the author proceeds to the anatomical description of the ovaries in the human species, which he finds to correspond with those of the Mammalia generally, and to consist of a *parenchyma* or *stroma*, and an envelope or *indusium*, derived from the peritoneum. The stroma immediately under the peritoneal envelope is condensed into the form of a tunic, to which the peritoneum closely adheres, and which has received the name of the *tunica albuginea*, or *indusium proprium*. The vesicles of De Graaf are imbedded in this tunic, and are situated principally near the surface of the ovary: in the human species they are about one fifth of an inch in diameter. The proper capsule of the Graafian vesicle is composed of two layers; the outer being thin, dense, and vascular; the inner, thicker, softer, and more opake. The nucleus of the vesicle consists of, 1st, a granular membrane inclosing: 2ndly, a coagulable granular fluid; 3rdly, a circular mass or disc of granular matter, termed by Baer the *proligerous disc*, connected with the granular membrane on the prominent side of the vesicle, and presenting in its centre, on the side towards the interior of the vesicle, a small rounded prominence, called the *cumulus*, and on the opposite side a small cup-like cavity, hollowed out of the cumulus; and, 4thly, the ovum, which is contained in the cavity just mentioned. The human ovum is so small as to be only just perceptible to the naked eye, being the 150th part of an inch in diameter. It has a soft transparent envelope of considerable thickness, and contains a substance composed of grains, adhering together by the intervention of a delicate mucous tissue. At the inner surface of the envelope, the author discovered a delicate transparent vesicle, about the 900th part of an inch in diameter, and having on one side a small elevation, which, projecting among the grains composing the walls of the granular sac, fixes the vesicle in its place. The author considers this vesicle as being analogous to that described by Professor Purkinje in the cicatricula of the immature eggs of birds, and which exists also in the ova of other oviparous animals, and is termed by Baer the *germinal vesicle.*

The author has also examined the ova of the cow, sheep, sow, rabbit, rat, and mouse, and has found in all these animals a germinal vesicle, differing in no essential particular from the human structure, and in size bearing a proportion to that of the ovum as one to six.

Although there is, at first sight, a considerable resemblance between the nucleus of the vesicle of De Graaf and the immature yelk of the egg of a bird, the author thinks, contrary to the opinion of Baer, that there is no real analogy between them; because, in the Graafian vesicle of the Mammalia there is no membrane surrounding its nucleus similar to the vitellary membrane of the ovum in birds, nor does this latter membrane appear first under the form of a granular membrane. The vesicle of Purkinje consists merely of a delicate

capsule containing a fluid; while in the minute ovum of Mammalia there are found all the essential elements of the egg of birds and other Ovipara, namely, an external membrane, analogous to the vitellary membrane, but performing a different function; a granular membrane, containing a thin fluid, corresponding to the immature yelk of a bird's egg; and a vesicle in every respect analogous to the vesicle which Purkinje found in the hen's egg, while still lodged in the ovary. The author considers the granular membrane, proligerous disc, and granular fluid of the Graafian vesicle, as parts which are superadded, and of which there is no trace within the capsule of the ovary of a bird.

"Some Remarks on the difficulty of distinguishing certain Genera of Shells; and on some Anomalies observed in the Habitations of certain Species of Mollusca." By John Edward Gray, Esq., F.R.S.

In opposition to the opinion of those geologists who consider all shells of the same form and character as having been inhabited by one genus of animals; that all the species of a genus live in similar situations; and that all the species of fossil shells, appearing from their character to belong to some recent genus, have been formed by animals which in their living state had the same habits as the most commonly observed species of that genus,—the author proposes to show, first, that shells having the appearance of belonging to the same natural genus are sometimes inhabited by very different animals; and, secondly, that some species of shell-bearing molluscous animals live in different situations from the majority of the species of the genus to which they belong, or even have the faculty of living in several different situations. Thus, although the animals inhabiting the shells belonging to the genera *Patella* and *Lottia* are extremely dissimilar in many essential features of their organization, the shells they form cannot be distinguished from one another by any known character. In other instances, when the animals are very different, the distinctive characters of the respective shells belonging to them are so slight as to be insufficient for the purpose of classing them under separate species; and this difficulty of discrimination must be much increased in the cases of fossil shells, especially of those which have no strictly analogous forms among recent shells.

In support of the position advanced in the second part of the paper, namely, that numerous exceptions occur to the identity of habitation among all the species of the same genus of conchiferous Mollusca, the author adduces examples: 1st, where the species of a genus are found in more than one situation, as on land, in fresh and in salt water; 2ndly, where one or more species of a genus, the species of which generally live in fresh water, are found in salt or in saltish water; 3rdly, where one or more species of a genus, which is generally found in the sea, are, on the contrary, found in fresh water; and, 4thly, where the same species of shell is found in salt and in fresh water.

"On the supposed Existence of Metamorphoses in the Crustacea." By J. O. Westwood, Esq., F.L.S., and Secretary to the Entomological Society. Communicated by J. G. Children, Esq., Sec. R.S.

The author refers the principal modifications of form which occur during the progressive development of animals to the three following

heads: 1st, that of an animal produced from the egg in the form which it is destined to retain through life, its only change consisting in a series of moultings of the outer envelope, attended merely by an increase of size, and not by the acquisition of new organs; 2ndly, when the animal, at its exclusion from the egg, exhibits the form which it continues to possess, subject to a series of moultings, during several of the last of which certain new organs are gradually developed; and, 3rdly, when the form of the animal, at its exclusion from the egg, is totally different from that under which it appears at the later periods of its existence; such change of form taking place during two or three of its general moultings, and consisting, not only in the variation of the form of the body, but also in a complete change in the nutritive and digestive systems, and in the acquisition of various new organs. This last phænomenon peculiarly characterizes what is termed a *metamorphosis*.

It is the received opinion among naturalists that the Crustacea do not undergo metamorphoses, properly so called, and that the transformations they exhibit consist merely in the periodical shedding of the outer envelope. The object of the present paper is to establish the correctness of this opinion, in opposition to that of Mr. J. V. Thompson, who has laid claim to the discovery that the greater number of the animals belonging to the class Crustacea actually undergo metamorphoses of a peculiar kind, and of a different character from those of insects. Mr. Thompson's views are founded upon some circumstances which he has observed in certain animals of the genus *Zoea* of Bosc, and which have been recorded by Professor Slabber, and which led him to believe that, of these animals, some were the young of the *Cancer Pagurus*, or common crab, and others the young of the *Astacus Pagurus*, or common lobster; and these views are supposed by him to be corroborated by the annual peregrinations of the land crabs to the sea-side for the purpose of depositing their eggs, rendered necessary by the aquatic habits and conformation of the young. The author then proceeds to examine at length the arguments on which Mr. Thompson has founded these opinions, and adduces his reasons for concluding that they are erroneous, and that no exception occurs to the general law of development in the Crustacea, namely, that they undergo no change of form sufficiently marked to warrant the application to them of the term *metamorphosis*.

"Memoranda relating to a Theory of Sound." By Paul Cooper, Esq. Communicated by J. G. Children, Esq., Sec. R.S.

The author, expressing his dissatisfaction with the commonly received theory of the propagation of sonorous undulations by an elastic medium, advances the hypothesis that each particle of an elastic body, after receiving an impulse in a particular direction, and communicating that impulse to the adjoining particle, instead of being thereby brought to a state of rest, is carried back by its elasticity with a velocity which continues its motion beyond the point from which it originally set out, and is thrown into continual vibration, in a manner analogous to the motion of a pendulum. He endeavours, on the principle of a continual transfer of the state of each particle to the adja-

cent particles, to explain the phænomena of continued sound arising from a prolonged succession of vibrations.

"A Theory of the Tides, including a Theory of the Formation and Propagation of Waves." By the same.

The author applies the principle announced in his paper on the Theory of Sound, namely, that of a continual transfer of state, between the adjacent atoms of a medium, to the case of oscillating columns of fluid, constituting waves and tides.

"On the Influence of the Respiratory Organs in regulating the Quantity of Blood within the Heart." By James Wardrop, Esq. Communicated by the Hon. Captain De Roos, R.N., F.R.S.

The author observes that the act of inspiration tends not only to favour the passage of the blood into the venæ cavæ, but also to detain it in the pulmonary vessels,—in consequence of the expansion of the lungs allowing of its more ready ingress into the pulmonary arteries, and impeding its exit by the veins—and thus retards its return to the heart. On the other hand, the collapse both of the lungs and of the parietes of the chest, during expiration, assists the transmission of arterial blood from the lungs into the left cavities of the heart, and promotes its passage into the aorta. Thus he considers inspiration as an auxiliary to the venous, and expiration to the arterial, circulation; the first acting like a sucking, and the latter like a forcing pump, in aiding the power of the heart. On this principle he explains the influence exerted on the circulation and on the action of the heart by various modes of respiration, whether voluntary or involuntary, in different circumstances. Laughter, crying, weeping, sobbing and sighing, &c., he considers as efforts made with a view to effect certain alterations in the quantity of blood in the lungs and heart, when the circulation has been disturbed by mental emotions.

The Society then adjourned over the long vacation, to meet again on the 19th of November next.

GEOLOGICAL SOCIETY.

(*Address of the President, G. B. Greenough, Esq., F.R.S., at the Annual Meeting, 20th February,—continued from p.* 152, *and concluded.*)

Two Communications have been presented to us, one from the pen of Mr. Babbage, the other of Captain Basil Hall, R.N., on the Temple of Serapis at Puzzuoli, one of the most extraordinary buildings in Europe; beautiful as a work of art, interesting as an object of antiquity, but to the geologist more especially valuable, as exhibiting a variety of complex natural phænomena, which, though they have taken place in times comparatively modern, it is exceedingly difficult to explain according to the known laws of nature.

Of the solutions which have been proposed by different authors*, not fewer than twenty in number, and most of them authors of eminence, it is impossible to give even a summary within the time al-

* Among these may be mentioned Barthelemy, Boué, Brieslak, Brocchi, Cochin, Billiard, Daubeny, Desmarest, Desnoyers, Forbes, Goethe, Hoff, De Jorio, Lyell, Pini, Prevost, Nicolini, Raspe, and Dr. Robertson.

lowed me. The merit of Mr. Babbage's paper, as far as original observation is concerned, consists principally in his notice of various stalactitical deposits, and his examination of their different characters and modes of production.

Mr. Babbage describes in detail all the appearances of this temple, and then inquires into the causes of the extraordinary revolutions which it must be admitted on all hands to have undergone: the principal difficulty, you are aware, is to account for the erosion of the columns by lithophagous animals, from the height of 11 feet to 19 above their base, the remaining parts being intact.

Mr. Babbage is of opinion that the building stood at first very nearly at its present level. Assuming that since that period it has both subsided and risen again, and that considerable changes have taken place in the relative levels of the land and sea in its vicinity, he explains these circumstances by supposing the edifice to have been built upon the surface of matter at a high temperature, which matter contracted afterwards by slow cooling; that at a still subsequent period a fresh accession of heat produced a new expansion, and that in this way the temple was gradually restored to its original level.

To suppose and illustrate his reasoning, the author has constructed a Table (founded on, experiments made in America,) showing in feet and decimals what would be the amount of expansion in beds of granite from 1 to 500 miles thick at various temperatures; together with a formula for calculating the amount of expansion in similar volumes of marble and sandstone; this Table has a collateral claim to notice, as being the first worked out by the calculating engine with a view to publication.

It appears to me, that in applying the calculation, it is very necessary to take into account three elements which have been overlooked.

1. How far under the supposed conditions expansion would be counteracted by pressure?

2. What space of time would be required to heat or cool such enormous masses of substances, which are very imperfect conductors of caloric, to the required temperatures*?

3. How far the explanation given of the phænomena of the Serapeum is applicable to others in its vicinity? The admitted fact that certain buildings which have also subsided still remain below the level of the sea, while others have been raised to unequal heights above it, makes it unlikely that any uniform cause, while it produced

* On a statement of Mr. Scrope's, that a current of lava after it had been ejected nine months, was still flowing on the flanks of Etna at the rate of a yard per day, Mr. de la Beche observes, "If lava can retain its elevated "temperature when thus exposed, what length of time may we not allow for "its doing so within the pipe of the volcano itself, surrounded on all sides by "matter greatly heated, and like itself an exceedingly bad conductor of heat? "Even in those cases where centuries elapse between the great eruptions "of any given volcano, the lava is probably liquid beneath at no very con"siderable depth."

upon them such various effects, should yet have stationed the pavement of this temple in the self-same spot which it occupied at the time of its original construction.

The letter from Capt. Basil Hall contains remarks on the position of the three columns of the temple which are still standing; they appear from his observations not to be exactly upright, but to bulge over a little, all in one direction, but not to the same degree. The outermost, in consequence of the tilt, has been brought into such a position that the top of the column is in a line with its base, an extraordinary accident. These remarks do not diminish our difficulties. The tilting may have been occasioned by such subsidences as all buildings are liable to, which are not founded upon solid rock, or it may be referred to earthquakes or original carelessness, or to the skill of the architect, who, by giving a deviation from the plummet line to the axis of the columns (so slight indeed as not to catch the eye,) strengthened his edifice against some lateral thrust, a practice known to have been employed at Athens, and referred to in the letter. Captain Hall has indeed put his veto on this last hypothesis, by saying that the inclination of the columns takes the opposite direction to that which would be required for the supposed purpose; but this cannot be known, I imagine, unless we know also the details of the original structure, and especially the position and construction of the roofs.

Shortly after this temple had been examined by Mr. Babbage, an attempt to drain it effectually was made by the Neapolitan Government: the stagnant water which infected the air of the neighbourhood was partly supplied from the mineral spring, partly from rain, partly from the sea: the experiment failed for reasons which it is not necessary to mention. Signor Nicolini, President of the Royal Academy of Naples, who was entrusted with the conduct of the work, and has published an account of it, discovered a rich mosaic pavement a hundred palms in length at the depth of sixteen palms below the level of the stagnant water, whence it appears that the sea must have risen sixteen palms since that pavement was laid. The existence of two pavements of different dates shows further that great changes of level took place before the present temple was built: but Signor Nicolini goes further, he advances a confident opinion that the level of the Mediterranean, in relation to the land, is even now changing.

In support of this doctrine, he not only refers to the phænomena of the temple of Serapis, but points out others in its neighbourhood, all tending to the same conclusion: he states, that in the year 1808 he spent ten days or more in sketching at this spot, and never once saw the pavement flooded, whereas during the last five years he has never once found it dry; that in 1790 the old road near the Serapeum being subject to be flooded, a new one was made at a higher level; and that at the commencement of the new road there is now visible, two palms below the sea level, the pavement of an old landing-place; that boats now pass near the promontory of Puzzuoli over a mass of tufa, which derived its name of "The Table,"

from having been formerly used as such by sea-faring people; that the ground floor of the Aspizio dei Capuccini is now under water; and that near Pizzo Falcone modern marks are seen on the tufa many inches under the level of the sea at low water.

Before I quit this branch of the subject, I would wish to suggest to future visitors of this temple, the following topics of inquiry.

What parts of the building have undergone repair? Can the date of these repairs be deduced from the nature of the materials employed, or the character of the workmanship?

Where is the pavement out of level, and to what amount? Are the subsided parts under the lines of thoroughfare, or can their sinking be traced to imperfect construction? Is the foundation such as an architect would call secure? Does it stand on stratum No. 6 of Mr. Babbage's section?

Were there roofs to the bath-rooms?

Would the fragments No. 6, 7, 8, form one column, or more than one? Was the original number of large (*cipollino*) columns greater than four?

Is the tufaceous deposit on No. 7 the same as that on the walls?

Are all the water lines horizontal?

Brick-work is found in the strata which buried the temple. What is the character of this brick-work? Is it reticulated?

Draw up a detailed and exact account of the strata.

What is the nature of the thermal spring? Evaporate a few gallons of the water, and send the deposit to the Society.

The plan which accompanies Mr. Babbage's paper being copied from that of Jorio, it is desirable, in order to prevent confusion and save expense, that this plan, with the numbers attached to it, should be adopted in any future description.

In the concluding part of the paper, Mr. Babbage proceeds to show in what manner existing causes may possibly elevate continents and mountain ranges, and a similar train of reasoning seems to have presented itself to Mr. De la Beche's mind about the same time. The justice, or at least the relevancy of the reasoning, depends on the establishment of many postulates which in the present state of our knowledge can be regarded only as matters of surmise: but I treated this subject so much at large on a former occasion, that I will not detain you with any further observations upon it now.

A Paper by Dr. Turner, our Secretary, informs us of some experiments which have been made on the action of high-pressure steam upon glass, and other siliceous compounds. The glass was suspended within the boiler of a steam-engine, encased in wire gauze at a temperature of about 300° commonly for ten hours a day. At the end of four months all the pieces were decomposed, and the plate-glass especially, consisting of silex and soda, was in some pieces corroded entirely through. Window-glass was less acted upon, and rock crystal wholly unaltered. Dr. Turner ascribes these changes to the influence of the water on the alkali of the glass, the white opake matter with which the decomposed pieces were coated being siliceous earth entirely free from alkali; but some portions of the silex also

must have been dissolved, for the apertures of the gauze were in some instances closed by a siliceous incrustation, and a small stalactite of silica was found depending from the lowest part. He points out the bearing of these results on the agency of water under high pressure on felspathic and other rocks containing alkalies, and in this point of view they are of great interest.

I hail with unfeigned pleasure the arrival of every paper which makes geology a science not merely of observation, but experiment. In the condition in which we stand at present, the geology of the laboratory is as essential to our progress as that of the open air.

The Metamorphoses of rocks which are so continually pressed upon our notice are capable of explanation by chemists only. Of those metamorphoses I will only observe, that they appear to me to be attributed too exclusively to Plutonic action. The phænomena which startle and delight us in the vicinity of whin dykes, we regard in Neptunian rocks without emotion.

The Account recently published by Professor Hoffmann of the marble of Carrara is very striking. The result of his examination is, that this pure saccharine limestone, in which no trace has been discovered of organic matter, although in its cavities are occasionally found pellucid crystals of quartz, is only transformed oolite. Mr. De la Beche's researches along the gulf of Spezia, an account of which is published in the Transactions of the Geological Society of France, had already prepared us for such an announcement: yet it seems strange when we reflect on the wide expanse of serpentine which is seen in its neighbourhood, that the Carrara marble should not be magnesian. In the Isle of Skye veins of serpentine sometimes penetrate the lias, where, in the vicinity of numerous whin dykes it assumes the whiteness and occasionally the sparkling grain of statuary marble, and here again the marble is unadulterated by magnesia: the origin of the serpentine is somewhat less mysterious, since the limestone in its unaltered state is micaceous. M. Dufrénoy in a late number of the "Annales des Mines," has described a similar Transformation of lias into saccharoid limestone seen in the Pyrenees. I think it unnecessary to detail to you the descriptions which Mr. Murchison has given of the Changes of structure, or even of substance, that take place at the Malvern Hills and at sundry other places in Wales, and on the confines of Wales, frequently, though not always, in the vicinity of trap and sienite, because they are in general the same as have been observed repeatedly in other districts. The phænomena at Old Radnor, the author remarks, are very analogous to those of the Val di Fassa in the Tyrol. It may, however, be proper to mention, not as a novelty, but as a circumstance the frequent occurrence of which is little attended to, that in Carmarthenshire the line of altered rock produced by the proximity, or as it is called the protrusion, of a mass of porphyritic trap, is parallel to the strike of the grauwacke so altered. At Caer Caradoc, the Wrekin, the Stiperstones, and elsewhere, a stratified sandstone is at its junction with trap converted into quartz rock. One other circumstance deserves to be noticed; the range of the Stiperstones,

along which these Plutonic appearances present themselves, is flanked on either side by metalliferous deposits, but not of the same kind, the copper ores being all found on one side of the range, the lead ores on the other. An analogous case will be seen in Humboldt's account of the country situate between the Oural and Altaic chains. A fault or fissure is there traceable through not less than 16° of longitude, forming a crest or water-shed; the rocks are nearly the same as those of Shropshire: they comprehend a granite (unconnected with gneiss), clay-slate, grauwacke-slate, augitic porphyry, and transition limestone, once compact but now granular. Malachite and red copper ore are found on one side of the ridge, argentiferous galena on both.

Such are the Plutonic phænomena, for an explanation of which we rely chiefly on the assistance of chemistry; but there is another train of phænomena which renders a close and intimate alliance between this science and our own no less desirable. The spontaneous generation, shall I call it? of agate, of chert, of hornstone, of flint, in the midst of sedimentary calcareous deposits, apparently through the instrumentality of animal or vegetable matter, in which little or no silex is to be met with, is one of those mysterious operations of nature which can nowhere be satisfactorily accounted for unless in the laboratory. The coralline agates of Antigua, the entrochal cherts of Derbyshire, the siliceous shells of Blackdown or Fontainebleau, the chalcedonic alcyonia of Pewsey, pieces of fossil wood either imbedded in strata or loosely scattered over sandy deserts, the flinty casts of echini and other substances in the midst of our chalk, all these suggest a course of experimental investigation which we are entitled to hope, if undertaken in earnest, would not be undertaken in vain.

Gentlemen, I have great satisfaction in announcing to you, that at the opening of the present Session of the Royal Society, one of the Royal Medals was awarded to our Foreign Secretary as the author of the most important discoveries or series of investigations sufficiently established or completed to the satisfaction of the Council within the last five years, and for which no honorary reward had been previously received. The Council of the Royal Society, premising that they decline to express any opinion on the controverted positions contained in Mr. Lyell's work, entitled "Principles of Geology," state the following as the grounds of their award.

1. The comprehensive view which the author has taken of his subject, and the philosophical spirit and dignity with which he has treated it.

2. The important service he has rendered to science by especially directing the attention of geologists to effects produced by existing causes.

3. His admirable description of many tertiary deposits, several of these descriptions being drawn from original observations.

Lastly, The new mode of investigating tertiary deposits, which his labours have greatly contributed to introduce; namely, that of determining the relative proportions of extinct and still existing spe-

cies, with a view to discover the relative ages of distant and unconnected tertiary deposits.

Of the Work so honoured by the Royal Society, the third edition has been lately published: in this edition some opinions formerly expressed have been modified or renounced, and much new matter has been introduced; the phænomena of springs and Artesian wells have been more fully treated; the theory of elevation has been entered into more at large, the author still controverting that theory. A chapter, almost entirely new, points out the probable causes of volcanic heat; objections are advanced against the doctrine of the central fluidity of the earth, and especially the intense heat attributed by some writers to the internal nucleus. Mr. Lyell considers how far chemical processes in the interior of the earth may generate volcanic heat, and what may be the effect exerted by currents of electricity. Sir Humphry Davy's theory of an unoxidated metallic nucleus is considered, and it is suggested that compounds resulting from the action of water upon metallic bases may be again deoxidated by the hydrogen set free in that process. The author has also given a more complete view of his opinions respecting the origin of caverns, and the manner in which they have been filled with breccia and the bones of animals. In illustrating this subject, he refers particularly to the recent discoveries of MM. Virlet and Boblaye in the Morea. His sketch of the principal secondary formations is also considerably enlarged.

Two other publications have issued from the press during the last year, which are eminently deserving of your attention. The first of these, entitled "A Treatise on Primary Geology," originated in great measure from a discussion that took place at a Meeting of the Geological Section of the British Association at Cambridge, and was drawn up with a view to the further consideration of the chief questions which it embraces, at the subsequent Meeting of the same body at Edinburgh.

Dr. Boase begins by describing the composition and relation of the several Primary Rocks, combining the accounts of geologists in various parts of the world with the results of his own laborious investigations in Cornwall. I regret, that within the limits to which I am restricted, it is impossible for me to do justice to the merits of this important work; I must confine my observations to a few of its most characteristic features. Dr. Boase is of opinion that the connexion between Unstratified and Stratified Rocks shows that they had a common and contemporaneous origin. He observes that granitic masses are as complex in their composition as stratified rocks, and form sometimes distinct regular beds, highly inclined and alternating with one another; that the elvans, or insulated beds of granitic rock, always partake of the nature of the containing slate, and have the same basis; that the difference between the granite and killas, or elvan, in Cornwall is often feebly marked, and still more feebly the difference between the granite and gneiss of Scotland and Germany; as little difference is there between the granite of the Alps and the talc slate adjoining. Where the granite changes its character, a cor-

responding change, he says, takes place in the slate. The elvans are connected on either side with the granite they intersect by the most intimate mineral gradations, or contain irregular portions or masses of the common granite, with which they coalesce; both are penetrated by crystals of felspar; both are striped with shorl. At Pednimerer Meer one of the parallel layers of granite runs through the elvan. In Scotland different beds of granite will intersect a common mass, and pass by minute mineral transition into one another, or into the characteristic granite of the district.

Dr. Boase considers that the Primitive Shisti are improperly said to be *stratified.* Pini has expressed the same opinion in two separate memoirs; the supposed planes of stratification are, in his view of the subject, mere transverse fissures. Prof. Phillips, Mr. Scrope, Dr. Fleming, Prof. Sedgwick, have all felt and expressed the difficulty of distinguishing in these shisti planes of cleavage, and planes of stratification. In the days of my geological apprenticeship I took great pains to observe and record dip and direction, and fondly hoped to obtain so large a number of accurate data on this subject as might enable me to arrive at last at some general and important result. I threw these data into tables, which only bewildered me. Suspecting the accuracy of my early observations, I repeated them again and again, guarding myself on every occasion more and more against probable sources of error; still I found my results perpetually varying, till at last my patience was exhausted, my clinometer discarded, my registers destroyed. Let it not be supposed, however, that my observations were useless; they taught me a salutary distrust.

Dr. Boase remarks that all the Slate rocks are composed of rhomboidal concretions, which are developed on a large scale by disintegration. Mr. Scrope had anticipated this remark, and generalizes it. He says the stratification of rocks of all kinds, where the strata are separated by seams, is produced by concretionary process.

Now, then, which of all the planes are the planes of dip? Dr. Boase, like the Woodwardian Professor, selects those which run with the laminæ, and yet the layers of massive crystalline and granitic rocks often lie the other way. But this seems to be very much a matter of taste; different observers selecting for the scene of their measurements different planes. Some pay great attention to the laminæ, others neglect them; nay, the same observer shall sometimes select as strata one series of planes, sometimes another.

Professor Phillips, in a passage which is too long to be quoted, has expressed the same idea in language equally expressive.

Dr. Boase presents to those who differ from him on this subject the following alternative: either Stratification implies a successive deposition of sedimentary matters held in suspension, in which case *none of the primary shisti are stratified*; or merely parallel planes without regard to the cause of their production, in which case *not only the primary shisti are stratified but granite also.*

In the thirteenth chapter will be found some excellent observations on the nature of Inclined Strata, tending to show that these last are

not necessarily the effect of disturbance, but are to be attributed, in the Primary Slates, to original structure, and in many of the Secondary, partly to this cause, and partly to deposition upon inclined surfaces.

The difficulty I have been considering is by no means confined to Primary Slates. Mr. Conybeare has observed on the coast of Sully, in Glamorganshire, that the Lias splits spontaneously into blocks of regular figure, corresponding to that of a crystal of calcareous spar. If this be the case, where are we to look for the seams of Stratification? I have felt for very many years, and I still feel, that the indistinctness of this term is one of the most dangerous stumbling-blocks we have to encounter. If we would found upon this distinction the grand classification of rocks into Neptunian and Plutonic, surely we ought to have some test by which to determine whether rocks are *stratified or not*. If, looking to the theory of M. Elie de Beaumont, we would know whether strata are conformable or disturbed, surely we ought to be placed in a condition to determine what *Strata* are. On taking leave, as I must do, of Dr. Boase's work, I again recommend it to your attentive perusal; it is written with great candour as well as earnestness, and will be found a useful corrective of many opinions which appear, to me at least, to have been too inconsiderately adopted.

Mr. De la Beche, one of your Vice-Presidents, to whose pen and pencil our science has been for a series of years continually and largely indebted, has published a small volume, entitled "Researches in Theoretical Geology". The main tendency of this volume is to establish the importance and practicability of subjecting geological opinions to the tests of chemistry and natural philosophy. The Author goes over much ground, keeping always the same direction, having apparently no other objects in view than the acquisition and communication of sound knowledge, the detection and exposure of error, and the discovery and establishment of truth. Unshackled by authority, unenslaved by preconceived opinions, unseduced by the love of novelty, free from all vanity of authorship, concise, methodical, exercising his judgement continually, his fancy seldom, the author may not obtain that popularity which with less merit he might have easily commanded; but such a work cannot fail to be appreciated here.

After taking a general view of the Solar System, and considering certain apparent agreements and disagreements in the condition of some of the Planets, Mr. de la Beche applies his observations entirely to the Earth, which he supposes to have been originally in such a state that its component particles had a free passage among one another. The principal Constituents of Land, Water, and Air, sixteen in number, are made up of Substances commonly termed simple: viz. oxygen, hydrogen, nitrogen, carbon, sulphur, chlorine, fluorine, phosphorus, silicium, aluminum, potassium, sodium, magnesium, calcium, iron and manganese. Adopting Laplace's hypothesis, that the sun, planets, and their satellites, have resulted from the Condensation of gaseous matter, he ascribes the Condensation of our own planet to the gradual Radiation of Heat into space. He shows how Sedimentary Rocks

may be deposited so as to present, from the first, inclined planes, and that we should therefore be cautious in referring to subsequent displacement all deviations from a horizontal level: he forms an estimate of the Destruction of Land by Breakers, of the wear and tear of Running Waters, of the transport of detritus by Rivers.

The mean Density of the crust of the earth is usually reckoned at 2·5. From a reference to the lists, which the author has drawn out, of the specific gravities of many rocks, of the various simple minerals which enter into their composition, and of certain recent shells, it would seem that 2·6 would be nearer the truth.

The Author investigates the Chemical Changes which Rocks undergo after their formation, and the subject of Concretions, such as *Ludus Helmontii*, &c. He remarks on the importance of attending to the Cleavage of Rocks, whether igneous or aqueous, and their Transformation: he considers the great Breaks of the Surface in reference to the effect which would result from its gradual cooling; and, from the contortions and Fractures of Mountain Chains, infers the Intensity of the forces that have acted upon them: he shows that certain Thermal Springs may be occasioned by the Condensation of volcanic discharges of gas and vapour, and ascribes the Uniformity of their Temperature to the Constancy of such condensation: he then treats of Volcanic Action and the gradual Rise of large tracts of Land.

When explaining the Formation of Valleys, Mr. De la Beche contends that the "Bursting of Lakes," as it has been termed, could not take place in the way supposed. The Area, comprised within soundings, that is, within the 100 fathom line, round the British islands, is delineated on a map, in order to show, that within that area at least, no Valleys are produced by Tides and Currents; whence it would follow, that such effects cannot be satisfactorily referred to such causes. Under the head of Faults, which are treated of at some length, the author shows with what facility "Craters of Elevation" may be formed, and expresses surprise that so much discussion should have taken place on so simple a case; he sees no difference between many Metalliferous Lodes and Faults.

The subject of Organic Remains is next investigated, and the Modification produced by various physical causes on the distribution of Life, particularly Animal Life in the sea. Diagrams are given to show that Shells, contemporaneously enveloped by rocks now forming, would probably not be of the same Species, even under the same parallels of latitude; but that the Species would be determined in some measure by the relative depth of the water at different places, on the influx of rivers, and other causes. Attention is particularly called to the manner in which Organic Remains may be entombed in a series of deposits along a gradually rising coast.

Under the head of Mineralization of Organic Remains, the Author shows that these bodies are not merely changed in character but in reality. One substance being substituted for another, a cast for an original, the Change varies in these bodies in proportion to their respective Solubility. Carbonate of lime being more soluble than phosphate, shells change much more rapidly than bones. Silica in shells

follows the same law as in agates, entering their cavities by infiltration.

The Author now gives a general Sketch of the various Rocks that are known to us; he remarks that the Primary differ chemically from the Secondary, and endeavours to account for the phænomena connected with animal and vegetable life, as exhibited in the several formations, upon the theory of a gradual Loss of Heat by Radiation. Upon this theory he would explain the Scanty Supply of Limestone in the earliest Rocks. The effect of great heat would be to expel from water the carbonic acid necessary to hold the carbonate of lime in solutions; and consequently Calcareous matter could not be deposited from heated water. He observes also that the Conditions for an uniform distribution of animal and vegetable life, would be more uniform in a thermal than in our actual Seas, and, therefore, if the Ocean had become gradually cooler, we should expect to find, as we do, genera and species more diversified.

The terms, Eocene, Miocene and Pliocene, are objected to, as prejudging an important question. Unless equal conditions obtained at equal times in all places, the Miocene rocks of one country may be of the same date as the Pliocene of another. The Author closes his remarks on Erratic Blocks by observing, that, like the great contortions and dislocations of strata, they teach us, while we duly appreciate the continued and more tranquil effects which are daily before our eyes, that we must not dismiss from our consideration Forces of greater Intensity; still bearing in mind, that however great these last-mentioned forces may be when measured by the ideas commonly entertained on such subjects, they are still insignificant when considered with reference to the entire spheroid on the surface of which they act.

Gentlemen, I have now brought to a close the account, which, in conformity with the practice of the Society, I proposed to lay before you, of the labours and achievements of last year. It therefore only remains for me to resign the chair. When I consented to resume the office of President, many of you are aware that a consciousness of the precarious state of my health made me diffident of my powers to discharge that office with becoming energy and effect. The generous support which I have received from the Council and the Fellows at large, has, I am willing to believe, in some degree concealed from your observation several deficiencies, of which I myself have been fully aware, and the Society has continued to flourish. The only merit I claim is, that of having, upon all occasions *endeavoured* to promote your interests; but a brighter prospect now opens upon you, and you will find an ample guarantee for more brilliant anticipations of success in the youth, the spirit, the abilities and the character of my successor.

ZOOLOGICAL SOCIETY.

February 10.—A Letter was read, addressed to the Secretary by W. H. Rudston Read, Esq., giving an account of the habits of the *Hyrax Capensis*, Pall., as observed at the Cape of Good Hope, and

also during a voyage thence to England, in a specimen brought home by the Rev. Mr. Hennah of H. M. S. Isis, which was presented to the Society after its death by Mr. Read. This letter is given at length in the 'Proceedings'.

Mr. Martin's Notes of the dissection of the specimen of *Hyrax Capensis*, presented to the Society by Mr. Rudston Read, were then read, and have also been printed in the 'Proceedings'.

"The dissection of the *Hyrax* by Mr. Owen ('Proceedings of the Committee of Science, &c.', Part II. p. 202.) is to be regarded as a confirmation of the anatomical details of this animal as given by Pallas, while at the same time it communicates several additional facts of grea tvalue. The present notes," Mr. Martin observes, "give nothing absolutely new; but may be of use as substantiating previous observations with regard to some very remarkable points of structure."

Some Notes by Mr. Martin, of the dissection of a *red-backed Pelican, Pelecanus rufescens*, Gmel., which recently died at the Society's Gardens, were also read. They refer to the male bird of a pair, the female of which was examined in 1832 by Mr. Owen, whose notes of the dissection were read at the last Meeting as recorded at p. 154.

"The osseous structure was light and thin, and the bones of the extremities were remarkable for the extent of their internal cavities and the thinness of their external walls. The *os furcatum* was largely spread, and firmly soldered to the keel of the *sternum*, keeping the shoulders widely apart. The clavicles, or what are regarded as the analogues of the coracoid processes in *Mammalia*, were large, and broadly expanded at their point of union with the *sternum*. The *sternum* was short in proportion to its breadth, measuring $4\frac{1}{2}$ inches longitudinally, and the same across, in a straight line, that is, not following the concavity of its inner surface: its keel was comparatively but little developed; it is thrown forwards, however, as far as possible, and projects in a point where it is ossified to the *os furcatum*. Its greatest depth is 1 inch 2 lines.

"The cervical *vertebræ* were 15 in number."

With reference to the bony union of the *os furcatum* to the *sternum* observed in this *Pelican*, Mr. Martin remarks that "in the *Adjutant, Ciconia Argala*, Vig., though the keel of the *sternum* is much more extensive, deep, and strong, the *os furcatum* much resembles that of the *Pelican*, and is in like manner ossified to its anterior *apex*. In the *common Heron, Ardea cinerea*, Linn., the *os furcatum* is feeble, but is also united by bone to the *apex* of the keel of the *sternum*: at its point of union a projection or short process is directed upwards; the keel of the *sternum* is here very ample. These are birds not so much of rapid as of untiring powers of flight, which, unlike that of the impetuous *Falcon*, is sweeping and majestic. In the *Falconidæ* the *os furcatum*, though very strong, does not at all approach to the form of a triangle, as in the birds alluded to, but describes a figure not unlike that of a horse-shoe, and a considerable space intervenes between it and the keel of the *sternum*."

Further details are given in the 'Proceedings'.

A Paper was read, entitled, "Characters and Descriptions of a new Genus of the Family *Melolonthidæ*: by John Curtis, Esq., F.L.S., &c."

In a collection of *Insects* recently received by the author from Lima is contained a beautiful series of the one constituting the type of his proposed new genus

ANCISTROSOMA.

Antennæ capite breviores.

Clypeus, in mare præsertim, emarginatus.

Thorax acutè marginatus, hexagonus; dente brevi in baseos medio armatus.

Pedes longissimi, robusti.

The stoutness of its legs and the sharp lateral edges of its *thorax* distinguish *Ancistrosoma* from all the neighbouring genera: the male is further characterized by an acute, rather long, and slightly curved spine near the base of the *abdomen* beneath. Its natural situation is probably between *Diphucephala*, Dej., the males of which have a bilobed *clypeus*, and *Macrodactylus*, Latr., which is very similar to it in habit, and has also very long legs; but these in *Macrodactylus* are slender, while in *Ancistrosoma* they are robust. Neither *Diphucephala* nor *Macrodactylus* possesses the little tooth at the base of the *thorax* lapping over the *scutellum*, a structure which is, however, met with in *Ceraspis* as well as in *Ancistrosoma*; but in *Ceraspis*, independently of the other differential characters, the *antennæ* and club are long.

ANCISTROSOMA KLUGII. *Anc. ferrugineum, suprà piceo-nigrum; thoracis margine elytrorumque strigis sex albidis.*

Long. maris 12 lin.; fœminæ plerumque minor.

Hab. in *Mimosæ* floribus apud Huanuco prope Lima, Peruviæ.

Of the three streaks on each of the *elytra*, the sutural one does not reach so far as the base, the second extends neither to the base nor to the tip, and the outer one is still shorter: they consist of broad punctured furrows, white with short hairs.

The cocoon of the *pupa* is ovate, hard, and in texture somewhat like that of *Trichiosoma Lucorum*, Leach; its *operculum* is semiorbicular, with a broad hinge and narrow rim: the shell of the *pupa* is similar to that of other *Melolonthidæ*.

Mr. Curtis describes in great detail the several parts of this *Insect*, and illustrates them by an extensive series of drawings, which were exhibited; as were also specimens of the *Insect* itself.

Mr. Curtis also communicated a Paper "On a species of *Moth* found inhabiting the *Galls* of a Plant, near Monte Video." The galls in question were collected by Mr. Earle (who accompanied Captain Fitzroy in H. M. S. Beagle,) in the month of December, about fifteen miles westward of the town, on a sort of underwood shrub, which Mr. David Don, on the examination of the small branches, and of a single leaf, thinks may probably be a species of *Celastrus*. Of the figures in illustration of the paper exhibited to the Meeting, one represents a branch supporting two of the galls, which are sometimes clustered five or six together. They arise where the attachment of leaves or flowers is indicated, and are therefore most proba-

bly produced by the transformation of the buds themselves, acted on by the *stimulus* of the insect secretions. On the side of each gall is a round aperture, with an *operculum* accurately fitted to it, which may easily be picked out with the point of a penknife. This *operculum* is equally convex on its outer surface with the rest of the gall, and is of the same thickness; but its internal diameter is less than that of its external surface, which forms a broader rim. Around the orifice the margin of the gall is thickened and a little raised. Within each of the entire galls was found a *pupa* attached to the base by its tail, with its head close to the *operculum*; which, it should seem, gives way by a slight expansion or elongation of the *pupa* when just ready to hatch, and the cast skin is left sticking in the passage.

Mr. Curtis observes that he was very much surprised to find on examination that the *pupæ* contained in these galls belonged not to the *Hymenoptera* but to the *Lepidopterous* order; an occurrence hitherto almost unprecedented. The characters of the *Insect*, as far as could be detected from the imperfect state in which it was found, are as follows:

CECIDOSES.

Caput parvum.

Antennæ corpus longitudine æquantes, graciles, ciliatæ, articulis elongatis numerosis: in capitis vertice prope oculos insertæ.

Thorax squamulis depressis vestitus.

Abdomen subrobustum, ovato-conicum.

Pedes longi; *tibiis* anticis spinâ prope apicem munitis, intermediis posticisque ad apicem calcaratis, his densè squamulatis et in medio præterea bi-spinosis; *tarsis* 5-articulatis, articulo basali longissimo; *unguibus pulvillis*que minutis.

Alæ sublanceolatæ.

CECIDOSES EREMITA. *Cec. cinereus; alis anticis saturatè brunneo maculatis, densè ciliatis; posticis albidis.*

Hab. prope Monte Video. *Pupa* in gallis *Celastri?* abscondita.

From the stoutness of the body Mr. Curtis is inclined to refer the *Moth* to the *Tortricidæ*; if belonging to *Pyralidæ* or *Crambidæ*, its *palpi* should be more strongly developed, but neither they nor the *maxillæ* were discoverable.

Figures of the imperfectly developed moth and of several of its parts, as well as of the galls and their *opercula*, together with specimens of the latter, were exhibited in illustration of the paper.

February 24.—A Letter was read from Lady Rolle, addressed to the Secretary, giving an account of the birth of two young *Monkeys*, the produce of a pair of *Ouistitis* (*Jacchus penicillatus*, Geoff.) in her Ladyship's possession. The parents were obtained in London during the last summer, and the young were produced on the 1st of January: one was born dead, but the other still survives, being about six weeks old. It appears likely to live, and is every day put on the table at the dessert, and fed upon sweet cake. Lady Rolle states that the mother takes great care of it, exactly in the manner described by Edwards in his 'Gleanings', p. 151, pl. 218; where the animal is figured and described under the name of the *Sanglin*.

It was observed that young of the same species had been born at the Society's Gardens, but not living; and that a female in the collection of the President, the Earl of Derby, at Knowsley, had produced, about the same time as Lady Rolle's, two living and healthy young ones, which are still thriving.

Mr. Gould exhibited a living specimen of the *red-billed Toucan, Ramphastos erythrorhynchus*, Gmel., which had recently come into his possession.

The exhibition was resumed of the new species of *Shells* contained in the collection of Mr. Cuming. Those brought at the present Meeting under the notice of the Society were accompanied by characters by Mr. G. B. Sowerby, which are given in the 'Proceedings'.

Genus Venus. Ven. *Columbiensis, subimbricata, undatella, discrepans, multicostata* (*long.* 4·3, *lat.* 2·7, *alt.* 3·7 *poll.*—perhaps the largest species known), *Peruviana, Australis,* and *spurca.*

Genus Cytherea. Cyth. *radiata* (belongs to that division of the genus which has four cardinal teeth, and is destitute of the cordiform anterior impression), *unicolor, concinna,* and *squalida* (bears some resemblance to *Cyth. maculosa*).

March 10.—Specimens were exhibited of several species of *Trogon*, partly from the Society's collection, and partly from that of Mr. Gould; and, at the request of the Chairman, Mr. Gould called the attention of the Meeting to some of the more interesting among them.

One of them was the *Bird* represented by M. Temminck, in his 'Planches Coloriées', under the name of *Trog. fasciatus*; and on this Mr. Gould remarked, that having had an opportunity of examining the drawing made by Forster, on which Pennant's original description was founded, he had ascertained that it represented a species altogether distinct from M. Temminck's *Bird*, and much more nearly resembling *Trog. Malabaricus.* As the name of *Trog. fasciatus* must necessarily remain with the species originally described under it, the one figured by M. Temminck requires another designation; and Mr. Gould proposed for it that of *Trog. Temminckii.*

Another, was the splendid species figured by M. Temminck, in the same work, under the name of *Trog. pavoninus*, a name by which it is now generally known; but on referring to M. Spix's 'Avium Brasiliensium Species Novæ', the original description and figure of *Trog. pavoninus*, Spix, appear to Mr. Gould to have reference to a totally different species, distinguishable by its smaller size, by the absence of crest from its head, by the comparative shortness of its hinder back plumes (which do not extend more than a few inches beyond the tail), and by the whole of the tail-feathers being black. The species exhibiting the peculiarities just adverted to will, of course, retain its original name of *Trog. pavoninus*; for the other Mr. Gould proposed that of *Trogon resplendens.*

Mr. Gould also characterized two species, hitherto apparently undescribed, as *Trog. ambiguus* and *Trog. citreolus.*

March 24.—A Letter was read, addressed to the Secretary by W. Willshire, Esq., Corr. Memb. Z.S., dated Mogadore, February 9,

1835, and referring to the skin of an *Aoudad, Ovis Tragelaphus*, Geoff., presented by the writer to the Society, and also adverting to his endeavours to obtain the animal, which, from the description of it given by the Arabs of the Desert, Mr. Willshire conceives must be the *Antilope Leucoryx* described by Pennant.

The exhibition was resumed of the new species of *Shells* contained in Mr. Cuming's collection. Those brought on the present evening under the notice of the Society, completed the genera *Venus* and *Cytherea*, which had been commenced at the Meeting on February 24. The *Shells* now exhibited were accompanied by characters by Mr. Broderip and Mr. G. B. Sowerby, which are given in the 'Proceedings', under the following names:

Venus *tricolor, histrionica, fusco-lineata, Chilensis, lenticularis, asperrima, costellata, opaca, variabilis, discors, Cypria* (a near relation to the Linnæan *Venus Paphia*), *crenifera, leucodon, Californiensis, compta, ornatissima, Mactracea, pulicaria, obscura.*

Cytherea *lubrica, alternata, tortuosa, affinis* (may be a variety of *Cyth. tortuosa*), *Dione*, varr. *β, γ, vulnerata, planulata* var. suffusa, *argentina, pannosa, pallescens, inconspicua, modesta, pectinata* (var. immaculata).

Specimens were exhibited of numerous *Thrushes*, chiefly inhabitants of the Himalayan Mountains and of India; and Mr. Gould, at the request of the Chairman, brought them under the notice of the Meeting, principally with the view of indicating those of the former district as constituting a new form in the family *Merulidæ*, Vig., for which he proposed the generic name

Ianthocincla.

Rostrum ferè ut in *Cinclosomate* et *Turdo* sed magis robustum: mandibulâ superiore ad basin setigerâ.

Nares basales, ovales, apertæ.

Alæ breves, concavæ, rotundatæ; remigibus 6tâ 7mâque longioribus, omnibus mollibus.

Cauda subelongata, concava, rotundata; rectricibus mollibus.

Tarsi elongati, robusti.

Hallux digitum medium longitudine subæquans, ungue forti subæquali munitus.

Typus genericus. *Cinclosoma ocellatum*, Vig.

Montium Himalayæ Incolæ.

The chief distinguishing characteristics of the genus *Ianthocincla* are the comparative length of the *tarsus*; the length of the hinder toe, and the great length of the claw by which it is terminated; the roundness, concavity, softness, and yielding character of the wings and tail; and the peculiar fullness, lightness, and downiness of the whole of the plumage, and particularly of that of the back and rump. The downy nature of the covering is alluded to in the generic name.

The following six species may be referred to it, viz.

Ianthocincla *ocellata* (Cinclosoma ocellatum, *Vig.*), *variegata* (Cinclos. variegatum, *Vig.*), *erythrocephala* (Cinclos. erythrocephalum, *Vig.*); and the following new species: Ianth. *squamata, chrysoptera, rufogularis.*

April 14.—Mr. Gould, at the request of the Chairman, exhibited, from the collection of the President, the Earl of Derby, a specimen of a species of *Toucan*, which he regarded as hitherto undescribed. It belongs to the same group with the other *grooved-billed Toucans*, and is consequently referrible to the genus recently proposed by Mr. Gould under the name of *Aulacorhynchus*. He pointed out the characters which distinguish it from the other species of the genus, and proposed for it the name of *Aulacorhynchus Derbianus*.

Mr. Gould remarked that the colouring of the extremities of the tail-feathers would alone suffice to distinguish from each other the four species at present known in this genus. In *Aul. sulcatus* the tips of the tail-feathers are not marked by any peculiar colour: in *Aul. Derbianus*, the two, and in *Aul. hæmatopygus*, the four, intermediate tail-feathers are tipped with brown: while in *Aul. prasinus* the whole of the tail-feathers are tipped with brownish red.

The exhibition was resumed of the hitherto undescribed *Shells* contained in the collection of Mr. Cuming. Those brought at the present Meeting under the notice of the Society were accompanied by characters by Mr. G. B. Sowerby. They consisted of the following species and varieties of the

Genus Monoceros. Mon. *imbricatum*, var.; *crassilabrum*, var. album; *costatum*; *cymatum*, Sow.; *acuminatum* (may be only a variety of *Mon. imbricatum*); *Globulus*; *punctulatum*; *unicarinatum*; and *citrinum*.

Specimens were exhibited of the following *Hymenopterous Insects*, partly from the collection of the Rev. F. W. Hope, and partly from that of Mr. Westwood. They were accompanied by characters by Mr. Westwood, which are given in the 'Proceedings'.

Plagiocera *apicalis*, n. s.; Prionopelma *viridis*, n. g.; Fœnus *australis*, n. s.; Thoracantha *flabellata*, n. s.; Campylonyx *Ampuliciformis*, n. g.; Trigonalys *melanoleuca*, n. g.; Diamma *bicolor*, n. g.; Meria *Klugii*, n. s., *Spinolæ*, n. s., *Millefolii*, *rufiventris*, *Latreillii*, and *dimidiata*.

We have indicated in this list the new genera and species; on the former Mr. Westwood makes the following observations.

Genus Prionopelma (Fam. *Chalcididæ*). Genus *Callimomem* (oviductu elongato) cum *Eupelmo* (pedibus intermediis) conjungens. Genus *Phlebopenes*, Perty (Del. An. Art. Bras., 3.), cum *Callimomi* fortè conjungendum.

Genus Campylonyx (Fam. *Proctotrupidæ*). A genere *Gonatopo* differt thorace continuo et alato, ab *Anteone* thoracis et antennarum structurâ.

Genus Trigonalys. Genus anomalum familiæ dubiæ. Caput et antennæ *Lydæ*, abdomen *Mutillæ*. Alarum nervi fere ut in *Myrmosâ* dispositi.

Genus Diamma (Fam. *Mutillidæ*). Genus *Myrmecodi* affine.

The following Notes, extracted by Sir Robert Heron, Bart., from his Journal, were read.

1814.—For a good many years I have attended to the habits of *Peafowl*, and for the last eleven have written down my observations.

I find the individuals to differ as much in temper as human beings: some are willing to take care of the young ones of others, whilst some have pursued and killed them, and this whether they had a brood of their own or not. Some cocks have assisted in the care of young ones, whilst others have attacked them. An early hen frequently has a brood herself the next year. Age makes no difference in the number of the brood. I have had six from a hen a year old, and one from an old hen. The hens have frequently a great preference to a particular peacock. They were all so fond of an old pied cock, that one year, when he was confined in view, they were constantly assembled close to the trellice walls of his prison, and would not suffer a japanned peacock to touch them. On his being let out in the autumn, the oldest of the hens instantly courted him, and obtained proofs of his love in my presence. The next year he was shut up in a stable, and the hens then all courted his rival; for the advances in these birds are always made by the female.

The japanned breed are, I believe, a variety originating in England. In Lord Brownlow's numerous breed of common, white, and pied, the japanned suddenly, in my memory, appeared amongst them. The same thing happened in Sir J. Trevelyan's flock of entirely the common sort; also in a breed of common and pied given by Lady Chatham to Mr. Thoroton: and in both cases to the extinction of the previously existing breed.

1821-2.—A black Poland cock, belonging to my friend and neighbour Mr. Kendall of Barnsley, was seized last winter, near the house, by a fox, but his screams being heard by the servants, he was rescued, desperately wounded, with the loss of half his feathers. In time the remainder of his feathers came off, and he is now become perfectly white. This seems to have some relation to the human hair becoming white at once from fear.

1827.—Mr. Reid, near York, has two Water Tortoises, brought over from the siege of Belleisle, which commenced in 1761: one of them, having wandered, was missing for sixteen years, when it was found on cleaning out another pond. They are both alive, and very tame.

1833, *April* 20.—This morning I found a large white Gold-fish in great distress. A large male toad had fastened itself upon the head and shoulders of the fish. On removing the toad, the fish swam away, apparently unhurt.

Colonel Sykes read a paper "On the *Quails* and *Hemipodii* of India," which he illustrated by the exhibition of a very extensive series of those *Birds*, belonging partly to his own collection, which was made in Dukhun, and partly to that of the Society, which has been enriched by specimens from various Indian localities.

The author prefaces his descriptions of the species by some general observations on generic distinctions and characters, and illustrates his remarks by commenting on some of the genera and species constituting the genus *Tetrao* of Linnæus and his followers. He shows that the form of beak alone is inadequate as a mark of generic distinction, and that the form, and number, and size of the toes and nails, are not always of themselves to be regarded as suffi-

cient for generic characters. Passing to the characters deriveable from the combined consideration of the beak and feet, on which Brisson's system was founded, he remarks on some incongruous associations which were thereby occasioned. Size, the most convenient mode (in his estimation) of distinguishing the *Quails* from the *Partridges*, cannot, he remarks, be admissible as affording adequate grounds for generic distinction. Habits, also, present many difficulties in defining associations into genera; those assigned by authors to an entire group belonging frequently to only one or a few of the species included in it, while in some cases, such as that of the *common Quail*, the habits differ in different localities; that bird being in Europe migratory, while in India (and probably in China also) it is stationary: its solitary habits, except at a particular season, are preserved in India, but its evident congener, the *Cot. textilis*, is never flushed without a second being found within a few paces. Plumage, although in many genera there is an evident tendency to assume a particular livery, is evidently unsuitable for general adoption as affording adequate grounds for generic distinction, however useful it may be in the discrimination of species.

After passing in rapid review the genera adopted by M. Temminck in the family of *Tetraonidæ*, and offering brief remarks on the validity of the several groups, Colonel Sykes proceeds to state that having felt himself disappointed in his attempts to form a just and precise estimate of generic differences from external characters only, he sought in internal organization, in the form of the tongue, and in the colour of the *irides* for additional guides and evidences of affinities or dissimilarities. As regards the former of these, he turned his attention principally to the stomach, the *cæca*, the proportional length of the *cæca* to the intestine, and the proportional length of the intestine to the body. Notes of these several particulars, as observed by him in India in nearly two hundred species of animals, are now in his possession; from which he extracts and arranges in a tabular form such as relate to the *Quails* and *Hemipodii*, and, by way of further illustration, such also as relate to some species of *Perdix*, *Francolinus*, *Columba*, and *Pterocles*.

Colonel Sykes then describes in detail the following species, accompanying his descriptions by observations on their habits, and on such other points connected with them as appear to him to be interesting.

Genus *Coturnix*.

1. *Coturnix dactylisonans*, Mey.
2. *Coturnix textilis*, Temm.
3. *Coturnix erythrorhyncha*, Sykes, in Proc. Comm. Sci. Zool. Soc., Part II. p. 153.—(Perdix, *Mey.*)
4. *Coturnix Argoondah*, Sykes, Ibid.—(Perdix, *Mey.*)
5. *Coturnix Pentah*, Sykes, Ibid.—(Perdix, *Mey.*)

Genus *Hemipodius*.

6. *Hemipodius pugnax*, Temm.
7. *Hemipodius Taigoor*, Sykes, Ibid., p. 155.
8. *Hemipodius Dussumier*, Temm.

XXVII. *Intelligence and Miscellaneous Articles.*

EXPLANATORY FACTS. BY MR. STURGEON, LECTURER ON EXPERIMENTAL PHILOSOPHY AT THE HONOURABLE EAST INDIA COMPANY'S ACADEMY, ADDISCOMBE.

PERHAPS a more disagreeable task could hardly fall to the lot of any individual than that of having to rescue from the unwarrantable attack of another, that which is justly and honestly his own right. And it is more particularly so, when the claim is either of a literary or a scientific character, where *credit* alone is but too generally the humble reward of much industry, anxiety, expenditure of money, time, &c.

I am, at present, however, placed in that unpleasant situation, and have no means so likely for redress as that of placing a few facts before the scientific part of the readers of the Philosophical Magazine.

By referring to the Number for November last, it will be seen that I had, some time previously, succeeded in producing electro-dynamic phænomena, of various classes, by giving to magnetically excited electric currents one uniform direction through the terminal conducting wires, by means of a certain contrivance which may very properly be called the *Unio-directive Discharger*; because it has the power of uniting, and discharging, in any *one* direction those currents which, in consequence of the mode of excitation, are originally urged, alternately, in directions opposite to each other.

Without some arrangement for this purpose, every magnetic electrometer in which coils of wire form the original source, would have remained comparatively useless; and those phænomena, the most interesting in electro-dynamics, could never have been produced by the opposing currents, however powerful, rushing from these copious sources of electric action.

To exhibit the spark, heat wires, or to produce the shock, it matters not in which direction the current flows, nor whether it reciprocates, or proceeds in one uniform direction.

Electro-magnetic phænomena may also be exhibited by reciprocating currents, or even by opposing currents, provided the force in one direction sufficiently predominates over that in the other. But in the production of chemical decomposition, with exact polar arrangement of the liberated constituents, it requires that the electric currents be not of a reciprocating character.

It is, moreover, a particular object of the experimenter, in every electro-dynamic process, to avail himself of as much as possible of excited electric force; and also to prevent, as far as he can, the existence of any counteraction whatever.

Now, in well-constructed magnetic electrometers, the reciprocating currents are nearly of equal force, and the predominancy, if any there be, can never be calculated on as a disposable force, either as regards *degree* or *direction*.

Besides, it would be exceedingly unscientific, in cases where power is wanting, to employ a *part* only, when the whole is available; or, as in the present instance, to employ the *difference* only, instead of the *sum* of the reciprocating electric forces.

Hence the obvious advantage of the *unio-directive discharger*, which places the whole of the excited force at the disposal of the experimenter, and gives to the magnetic electrometer a degree of importance which it could never have possessed without it.

Now, it appears by a paper in the Phil. Mag. for March last, that my friend Mr. Watkins had become somewhat alarmed by the publication of my experiments which were made at the Adelaide Exhibition Rooms, thinking, no doubt, that it was an encroachment on some claims which, he imagined, were due to himself and Mr. Saxton.

I was well aware, from the best sources of information, that no apparatus for collecting and giving a proper direction to the excited currents, had ever been applied to the magnet in the Adelaide Rooms previously to my employing it on the evening of the 28th of August, 1834. I had also the satisfaction of ascertaining from Mr. Maugham, chemical lecturer, who so kindly assisted me, and also from the assistant present on that occasion, that no results like those produced by my experiments had ever been known amongst them. And it is singularly fortunate, for the cause of truth, that even to this day no apparatus in the capacity of an unio-directive discharger has ever been attached to that splendid magnetic electrometer, with the solitary exception of that *humble substitute* which I employed on that occasion.

Indeed, the claims of Mr. Watkins have, from the first reading of his paper, appeared to me, to be of that futile description which, in his ardour to excel, he has himself eventually shown them to be.

The Phil. Mag. for the present month shows very clearly that Mr. Watkins has not made even the slightest alteration, consequently no improvement, in the original apparatus! "Hence," says that gentleman, "we have in every slow revolution two actions and two reactions; one of these actions, it is true, tends to the same direction as one of the reactions; but still we have two directions of the current, and these two are antagonizing, therefore we have no TRUE POLAR decomposition," &c. (p. 111.)

My only business, at present, with this curious passage, is that of directing the reader's attention to one unsophisticated fact; and no words, I imagine, could express more clearly than these do, that Mr. Watkins has never yet accomplished "TRUE POLAR decomposition" by magnetic electricity; nor is it possible to keep separate the liberated constituents of any compound, by the form of apparatus which he appears to have employed.

The results of all such experiments as those Mr. Watkins claims must ever be fortuitous, as it would be impossible to predict the direction in which the predominating force, by any new machine, would be exerted.

Very different indeed are the results from the unio-directive discharger. Electro-dynamic phænomena, by this apparatus, are exhibited from revolving coils, either with or without a ferruginous nucleus or armature, with the same promptitude, uniformity, and precision as by any other source hitherto placed in the hands of the philosopher.

The experimenter also, by this means, may safely confide in his predictions, and vary his exhibitions in any way he pleases, as far as the energy of the currents will permit. He is thus relieved from all those corroding apprehensions and mortifying disappointments, which must ever molest his efforts, agonize his feelings, and chill the ardour of his inquiries, whilst operating with an apparatus over the powers of which he has not the slightest control.

That these facts might appear unfettered and without disguise, I have rigidly abstained from touching on any of the assailable points which are distinguishable in Mr. Watkins's last paper, with the exception of those which appeared absolutely necessary for placing in a proper light the obvious distinction of our claims.

The following particulars may possibly be interesting to those engaged in magnetic electricity.

I produce electric shocks, sparks, steady deflections of the needle, electro-magnetic rotations, &c., and chemical decomposition with exact polar arrangement of the liberated constituents, by the following forms of magnetic electrometers:

1. By revolving coils of copper wire between the poles of either a horse-shoe or a compound bar magnet, so as that the wire may strike, at right angles, the most formidable group of magnetic lines, as shown in my theory of magnetic electricity. (See Lond. and Edinb. Phil. Mag., vol. i. p. 31.)

With the exception of my revolving discs, described in the Phil. Mag. for April 1832 (Phil. Mag. and Annals, N.S., vol. xi. p. 270.), this is the oldest of my magnetic electrometers. But for want of a sufficiently powerful magnet it was a long time before I had much satisfaction from it. I have more recently been better provided, and I find that it acts well; and appears to me to be better calculated for some points of inquiry than any other form I have yet seen.

2. By revolving coils of wire (having an iron axis or armature) in front of the poles of a horse-shoe magnet. My first revolving armature was simply a straight piece of iron carrying a coil of wire, and revolving in a horizontal plane above the poles of a vertical electro-magnet. The idea of this form occurred whilst Mr. Watkins was describing to me the well-known apparatus of M. Pixii, an account of which had reached him a short time before. With this form I never did anything more than produce a feeble spark.

In the autumn of 1833, I first saw, in its present state, the splendid apparatus in the Adelaide Exhibition Rooms, made by Mr. Saxton. This modification of M. Pixii's electrometer far exceeding in power my puny arrangement, I have, from that time, employed bent armature and two coils in the manner of Mr. Saxton.

3. Bent armature and coils similar to No. 2. The magnet vertical, and the revolving axis carrying the armature and coils at right angles to the plane of the magnet.

4. Similar to No. 3, with the exception of a second piece of armature, with its coils, revolving on the same axis on the opposite side of the magnet. The greatest power is obtained when the pieces of armature are placed at right angles to each other.

5. By fixed coils on the two branches of a horse-shoe magnet, and a short thick piece of soft iron revolving in front of the poles. This is a very neat form.

6. By four cylinders of soft iron, with their coils, permanently fixed to the poles of the magnet, one on each side of each pole. The excitation is carried on by a revolving piece, as in No. 5.

My *unio-directive discharger*, which can be applied to any of the above forms, is by far the most happy contrivance I have yet hit upon in this class of apparatus. It consists principally of four or more semicylindric pieces, properly attached to a revolving spindle.

The mercury, which has hitherto held so distinguished and important a situation in the discharging part of magnetic electrometers, but which is a complete nuisance to the operator, I have, in most processes, entirely dismissed, by the introduction of my newest forms of discharger.

Artillery Place, Woolwich, Aug. 10, 1835.

ON A METHOD OF ASCERTAINING, APPROXIMATELY, THE RADII OF CURVATURE OF UNEQUALLY CONVEX LENSES. BY N. S. HEINEKEN.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

If the following simple *practical* method of ascertaining the radii of curvature of convex lenses appears to you to be worth insertion in the Philosophical Magazine, it is at your service.

I am, Gentlemen, yours, &c.,

N. S. Heineken.

In order to ascertain the radii of the curvatures of *crossed* or unequally convex lenses when *great accuracy* is not required, the following simple practical method may be used, *provided* the radii are not great: let the end of a piece of black or red sealing-wax be a *little* softened by holding it near a candle; breathe slightly upon the lens, press the softened wax upon it, and let it remain until cold. Upon removing the wax, it will be found, of course, to have taken the form of the lens; it will, in fact, have become a kind *of mirror*, whose concavity will correspond with the convexity of the surface of the lens. This *mirror* will be sufficiently brilliant to form the image of a candle or the sun, by reflection, upon a piece of card or white paper held before it. Now, the distance between the centre of this mirror and the image thus formed will be equal to half the radius of its curvature, and consequently to half the

radius of the curvature of the lens in question. In like manner the curvature of the other surface of the lens may be discovered. I have found this method useful in ascertaining the radii of the surfaces of lenses forming the eye-piece of telescopes and microscopes, to which, as they are of *short* foci, it is more particularly applicable. I have mentioned *sealing*-wax, as being the material most *easily obtained* for the experiment; but I have found *fusible metal* to be far preferable. In employing this, however, there is some chance of injuring the lens, in consequence of the greater degree of heat employed. The mode of using the metal (as in the French method of forming medallions) is to melt it with as little heat as possible, and pour it upon a piece of paper; then, as soon as, in cooling, it begins to get *pasty* at the edges, to stamp the lens upon it: a tolerably brilliant mirror will be the result.

Will you allow me, as a postscript to this communication, to make the following inquiries? In the Edinburgh Philosophical Journal for April 1823, it is stated that the late Sir William Herschel had composed a work on the subject of grinding and polishing specula for telescopes, and that it was his intention to publish it. Was his intention ever carried into effect? If not, may the scientific world *still hope* to possess the work of one so eminently qualified to instruct others in a difficult, and, I believe, little understood mechanical operation? In connexion with the same subject, Sir John F. W. Herschel mentions, in his letter from the Cape, contained in your Number for June, that he had taken out with him "a complete *polishing apparatus* with the efficacy of which he was perfectly satisfied." Now, as the *ordinary process of polishing*, and giving a *correct figure* to, the specula of telescopes is most *tedious* and *uncertain*, can any of your correspondents furnish a description of so valuable an apparatus, or could Sir John Herschel (when unoccupied by more important engagements) be prevailed upon to make known the construction of the engine to the public?

Collumpton, April 18, 1835.

ARSENIC IN ENGLISH SULPHURIC ACID.

Vogel of München infers from his experiments on sulphuric acid:

1. That the Nordhausen acid, prepared from the sulphate of iron, contains no arsenic; the precipitate with sulphuretted hydrogen being pure sulphur.

2. Concentrated English sulphuric acid prepared in leaden chambers contains arsenic, and the precipitate produced in it by a current of sulphuretted hydrogen consists of sulphur and orpiment.

3. No precipitate of sulphur takes place, in consequence of a current of sulphuretted hydrogen being passed through English sulphuric acid, diluted with from four to six parts of water; the precipitate consisting of an orange yellow powder, or orpiment.

4. Rectified English sulphuric acid contains no arsenic; this substance remaining in the residue. The rectified acid diluted with water is not rendered muddy by sulphuretted hydrogen. The

German sulphuric acid diluted with water becomes white when the latter gas is passed through it, as it always contains sulphurous acid.

5. The arsenic always exists in sulphuric acid in the form of arsenious acid, never as arsenic acid.

6. Concentrated boiling sulphuric acid can dissolve one third of its weight of arsenious acid, of which the greater part separates on cooling. The arsenious acid may be precipitated in a great measure from the concentrated sulphuric acid when cooled, by absolute alcohol, although it is somewhat soluble in alcohol.

7. Lastly, it is absolutely necessary that in all preparations to be used internally, rectified, or at least German sulphuric acid should be employed.—*Thomson's Records of Science,* July 1835.

APPEARANCE OF HALLEY'S COMET.

Communications have appeared in the daily journals from our correspondent the Rev. Dr. Hussey, and from Sir James South, from which we derive the following particulars of the discovery of Halley's Comet, in its present approach to the Sun, by those astronomers.

The Rev. Dr. Hussey observed the Comet at the Rectory, Hayes, Kent, on the mornings of Sunday and Monday, August 23 and 24, and gives for its approximate place Æ $5^h\ 42^m\ 20^s$, N. Decl. $23°\ 45'\ 20''$; stating it to be very large, but the faintest object the eye can distinguish, in an achromatic telescope of 6·5 inches aperture; and comparing it, in another communication, to the finest smoke.

Sir James South also observed the Comet at his Observatory at Kensington, in the form of a round, well-defined, but extremely faint nebulous body, perhaps 2 minutes of space in diameter, on Sunday morning, August 23, at 1 hour 11 minutes sidereal time; in about Æ 5 hours 42 minutes 31 seconds; and N. Decl. $23°\ 43'$. On Monday morning, August 24th, at $23^h\ 55^m\ 47^s$ sidereal time, its place was Æ $5^h\ 43^m\ 18^s$, N. Decl. $23°\ 49'\ 43''$.

The place assigned to it in the Nautical Almanac, for August 23, is Æ $5^h\ 42^m\ 56^s$, N. Decl. $24°\ 45'{\cdot}3$; so that, as Mr. Lubbock has remarked, the agreement with calculation of its observed place is as near as could have been hoped for.

It is stated in a foreign journal, that a letter had been received from Prof. Encke, announcing that the Comet had been discovered by M. Kunowski at Berlin on the 22nd of August.

In addition to MM. Damoiseau, Pontécoulant, and Rosenberger, who have each gone through laborious calculations requisite to predict the return of this Comet, Dr. Lehrman has also investigated this question and arrives at very different conclusions. According to this mathematician the Comet will pass through Ursa Minor, and the time of the perihelion passage which he assigns is ten days later than that previously determined. Dr. Lehrman has given his results in two recent numbers of the *Astronomische Nachrichten*.

CONGRÈS SCIENTIFIQUE.

The third meeting of the *Congrès Scientifiques* is to be held at Douay on the 6th inst. The following extract from a letter with which we have been favoured by a friend, now in France, will give our readers fuller information on the subject.

"Their first meeting, at Caen, appears to have been very similar in its plan and objects to the meetings of the British Association for the Advancement of Science. I have not seen the account of the second meeting, which was held last year at Poitiers. M. Caumont (who has taken a very active part in the formation of the *Congrès Scientifiques* of the French, and has indeed done for them what Mr. W. V. Harcourt did for us,) expressed a very strong desire that the English savans should meet them at Douay; and one of my objects in writing is to request you to put a notice of the meeting in your Journal for next month. *It will be commenced at noon, September 6th*, in the *Palais de la Cour Royale, Douay*. I should be glad if you could give the information to any of the friends of science in our country who might be likely to attend. Douay is only a very easy day's journey from Calais.

"Supposing that the French have, as we have, a permanent council, or any permanent officers, we and they may assist each other in the following particulars. That the members or officers of the one Association should give information to those of the other, of the time and place of the meetings. That the Secretaries should assist each other by inserting advertisements, &c., in the journals, newspapers, or other publications, notifying the time and place of meeting, and any other circumstances. That the two Associations should exchange their annual Reports or other publications, each disposing of the copies of the other's publications to the best advantage, or giving advice as to the public libraries, institutions, or journals and reviews, to which they should be sent. Lastly, that they should communicate together on any scientific researches, which it is their object to pursue, such as those relative to meteorology, the tides, &c."

DUBLIN MEETING OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The Fifth Meeting of the British Association, which was held in Dublin during the last month, has afforded abundant gratification to those who were able to attend, and will, we are fully persuaded, in various modes materially promote the great object of the Institution—the advancement of science.

Details of the kind attention and cordial and munificent hospitality of our Irish friends towards those who were assembled in Dublin, and of various matters which may be considered as accessories to such a meeting, have been given in the daily and weekly papers. The appropriate province of our journal will be to record a more full and exact account of the proceedings of the Association than hasty reports can supply; and this we hope to do next month to the satisfaction of our readers, as in conformity with a resolution of the Association the official reports of the Sections are to be furnished to us and to our contemporaries by the Secretaries.

SUCCINIC ACID AND ITS COMBINATIONS.

M. F. Darcet read to the Academy of Sciences an Essay on the above subjects.

MM. Liebig and Wöhler had ascertained by analysis that succinic acid contained only half an atom of water, and that this proportion appeared indispensable. This difference from organic acids, such as lactic acid, which are similar in other respects, was deserving of being carefully verified; and for this purpose M. Darcet undertook the requisite experiments. He observes that common succinic acid loses a definite quantity of water by sublimation; but this quantity becomes irregular when it has been rapidly distilled once or twice, and it loses more, according to the number of distillations to which it is subjected, so that it may be obtained in an anhydrous state.

The distillation carried on with a substance having great affinity for water, such as phosphoric acid, is much more rapidly effected and less of it is decomposed.

Succinic acid is soluble in water, and much more so in hot than in cold water; and thus its solutions readily crystallize on cooling: it is soluble in alcohol, but scarcely so in æther. When it is extremely pure, it fuses at 356° Fahrenheit; at about 284° it loses half an atom of water, and furnishes an acid that contains only half an atom, which crystallizes in fine needles: its boiling point is 455°. Its composition is, when anhydrous,

Carbon........	41·15	or	C^8 306
Hydrogen	5·49		H^6 37·5
Oxygen	53·36		O^4 400
	100·		743·5

Succinate of silver, obtained by double decomposition, as by pouring neutral nitrate of silver into neutral succinate of ammonia, both heated to 140° Fahrenheit, was found to be composed of

Succinic acid	30·39
Oxide of silver	69·61
	100·

It is consequently composed of an atom of anhydrous succinic acid, and an atom of oxide of silver: its atomic weight is = 2082·6. When submitted for a long time to a heat of from 234° to 252° Fahrenheit, common succinic acid undergoes a remarkable change; there are gradually formed in the neck of the retort perfectly white and fine slender needles, whilst half an atom of water is lost by the acid. On distilling together 16 parts of succinic acid, 20 parts of alcohol, and 5 of concentrated muriatic acid, and cohobating the liquor four or five times, there is obtained in the retort a yellowish liquid of an oleaginous consistence, composed of alcohol, water, succinic acid, muriatic acid, and succinic æther: on the addition of water, small drops of an oily liquid are precipitated; they are of a dark-brown colour, and soon unite at the bottom of the vessel. This is impure succinic æther: in order to render it perfectly pure, it is sufficient to wash it several times with cold water, and to heat it, until its

boiling point is constant, and then to distil it from oxide of lead; by these means a limpid colourless liquid of a sharp and burning taste is obtained, and a smell which resembles that of benzoic æther. It burns with a yellow flame, is oily to the touch, boils at 317° Fahrenheit, and its specific gravity is 1·036.

This substance analyzed by oxide of copper gave

Carbon	56·66	$= C^{16}$
Hydrogen............	7·95	$= H^{14}$
Oxygen..............	36·39	$= O^{4}$
	101·	$= C^{16}\,H^{14}\,O^{4}$

This æther when treated with potash yields alcohol; when submitted to the action of chlorine, succinic æther is decomposed; in diffused light the reaction goes on very slowly, but solar light immediately determines it. The chlorine disappears, and is replaced by muriatic acid: there are very soon deposited upon the sides of the bottle numerous crystals of succinic acid, mixed with a viscid yellow matter.

Ammoniacal gas has no action upon it; but when shaken with liquid ammonia, the succinic acid soon disappears, and after some hours a white crystalline matter is precipitated, which appears to have some analogy with oxamithon.

The density of the vapour of succinic æther is 6·22; calculated according to its formula, we have

16	volumes vapour of carbon	6.75
14	——————— hydrogen	0·96
4	——————— oxygen	4·41
		12·12

The $\frac{2}{12\cdot12} = 6\cdot06$.

Succinamide. When ammoniacal gas is heated and made to act upon succinic acid, a perfectly white substance is obtained; not possessing any of the properties of the acid, crystallizing with great readiness in very regular rhombs, and more soluble in alcohol than in water. When treated with potash at a high temperature it loses its ammonia. When succinate of lime is submitted to heat, a substance is obtained which M. Darcet calls *succinone.—Journal de Pharmacie,* No. xl.

NEW LOCALITY OF PLENAKITE.

Plenakite has been found in very perfect crystals, accompanied by quartz, in the brown iron ore of Framont. Its specific gravity is 3·00. Hardness equal to that of topaz. It cleaves readily parallel to the faces of a rhombohedron of 116° 40′, according to M. Beirich. It has been analysed by Prof. G. Bischof of Bonn, who finds that its composition is expressed by the formula $\dddot{\mathrm{B}}\mathrm{e} + 2\,\dddot{\mathrm{S}}\mathrm{i}$.—*Poggendorff's Annalen.*

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London, and by Mr. VEALL, at Boston.

Days of Month. 1835.	Barometer. London. Max.	Barometer. London. Min.	Barometer. Boston. 8½ A.M.	Thermometer. London. Max.	Thermometer. London. Min.	Thermometer. Bost. 8½ A.M.	Wind. Lond.	Wind. Bost.	Rain. Lond.	Rain. Bost.
July 1	30·099	30·073	29·60	77	52	64	S.	E.	...	...
2	30·087	30 033	29·54	80	52	64	S.	E.	...	...
☽ 3	30·092	30·069	29·47	79	52	62	SW.	calm	...	...
4	30·098	29·974	29·47	78	52	64	W.	calm	0·24	...
5	29·930	29·841	29·25	72	52	60	W.	calm	·01	0·49
6	30·164	29·938	29·39	73	44	62·5	SW.	NW.	...	·38
7	30·168	30·045	29·56	72	52	62	SW.	NW.	·10	...
8	30·057	29·981	29·32	70	45	64	SW.	W.	...	·07
9	30·015	29·744	29·36	67	55	63·5	S.	W.	...	...
○ 10	29·964	29·794	29·16	71	48	61	SW.	W.	...	...
11	30·101	29·069	29·50	72	48	60	W.	W.	...	...
12	30·006	29·833	29·41	75	54	58	SW.	calm	·02	·04
13	30·007	29·853	29·21	71	49	63	SW.	W.	·02	·06
14	30·057	30·022	29·45	72	43	61·5	W.	W.	...	...
15	29·917	29·856	29·27	75	56	66	S.	W.	·02	...
16	29·997	29·924	29·33	78	48	63	W.	calm	...	·16
☾ 17	30·057	30·043	29·40	83	49	66	SW.	W.	...	...
18	30·027	29·946	29·32	83	49	68	S.	W.	...	...
19	30·157	30·144	29·47	80	46	64·5	S.	calm	...	...
20	30·172	30·148	29·46	84	54	65	SW.	W.	...	...
21	30·209	30·184	29·56	89	55	68	E.	E.	...	...
22	30·249	30·209	29·68	76	59	62·5	E.	E.	...	...
23	30·275	30·230	29·72	80	51	64	E.	E.	...	...
24	30·202	30·165	29·65	82	54	68	E.	E.	...	...
● 25	30·158	30·136	29·59	80	54	68	E.	E.	...	...
26	30·160	30·086	29·60	80	53	69	SE.	E.	...	...
27	30·057	30·031	29·47	85	50	70	E.	E.	...	...
28	30·124	30·052	29·42	91	54	66	SW.	calm	...	...
29	30·210	30·178	29·58	85	48	64	SW.	calm	...	...
30	30·180	30·069	29·47	88	50	67	N.	calm	...	...
31	30·128	30·097	29·49	78	46	61·5	NE.	calm	...	...
	30·275	29·744	29·45	91	43	64·2			·41	1·20

Remarks.

London. — July 1. Fine. 2. Sultry, with thunder. 3. Very fine. 4. Very fine: rain at night. 5. Cloudy: slight showers. 6—8. Fine. 9. Overcast. 10—15. Fine. 16—20. Very fine. 21—26. Hot and dry, with wind easterly. 27. Cloudless and excessively hot and dry. 28, 29. Sultry. 30, 31. Clear, hot and dry. The quantity of rain which fell in the early part of the preceding year was below an average; and, in consequence of the great heat, the unusually large quantity of 6·34 inches which fell in July was not experienced as a superabundance in regard to vegetation. Scarcely one fifteenth part of the above quantity has fallen in this month, being little more than four tenths of an inch; and so limited a quantity in July has, perhaps, seldom been recorded. Happily the corn and hay crops had a plentiful supply from the rains in May, from which they derived sufficient moisture till they attained perfect maturity; otherwise, after a dry April, a partial failure must have ensued.

Boston.—July 1, 2. Fine. 3, 4. Cloudy. 5. Rain. 6. Fine. 7. Fine: rain P.M. 8. Fine. 9, 10. Cloudy. 11, 12. Cloudy: rain P.M. 13, 14. Fine. 15. Fine: rain P.M. 16—21. Fine. 22. Cloudy. 23—27. Fine. 28. Fine: foggy 7 A.M. 29. Cloudy. 30, 31. Fine.

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XXVIII. *On a new Method of reducing Lunar Observations for the Determination of the Longitude.* By CHARLES RUMKER, *Esq., F.R.A.S.**

To Lieut.-General Sir Thomas Macdougall Brisbane, K.C.B., F.R.S. L. & E., F. Ast. S., &c.

SIR,

THE lively interest which you took in the determination of the longitude by lunars, and the success which attended your observations, prompt me to propose to you the following method of their reduction, which admits of greater accuracy when either altitude is low. Already when I had the honour of accompanying you on your passage to New South Wales, I had an opportunity of remarking the necessity of a more correct allowance for the refraction of the lower object. This subject has since been resumed by a celebrated astronomer, whose method appears to me, however, not likely to be generally adopted by seafaring men, requiring, moreover, a particular ephemeris, which becomes inapplicable under ordinary circumstances, where the usual methods are sufficiently correct. I trust, therefore, that the following method, which requires no other ephemeris than the Nautical Almanac, and may, as circumstances require, be computed with more or less precision, will be found of more practical utility.

The object of the present lines is the correction of the error committed in the usual methods of clearing the apparent distance between sun and moon by taking out the refractions

* Communicated by Sir T. M. Brisbane, K.C.B. &c.

for their central altitudes, whereas it is the refraction of those points of their limbs whereof the distance is actually measured that should be used in the calculation. As introduction, it may not be useless to remind, that the usual methods of clearing the distance, which suppose the altitudes known by observation, can be classed in direct and approximatory ones. For as all the former methods are derived from the equation existing between the sides of the two triangles formed by the apparent zenith distance and apparent distance, and the true zenith distance and true distance, and only differ by slight variations in the manner of finding thence the true distance, or side of the latter triangle opposite to the angle at the zenith which is common to both triangles, thus all the latter methods agree in the former or apparent triangle, the angles at the sun and moon, and the sides adjacent thereto in two right-angled triangles; having for hypothenuses the corrections of the sun's and moon's altitudes, these sides are the corresponding corrections of the distance by a first approximation.

Let D designate the apparent distance of centres, S and M the above angles at the sun and moon, $\varrho-\pi$ the differences between parallax and refraction or corrections of altitudes, then is the true distance of centres $= \mathrm{D} + \text{cosine S}.(\varrho-\pi) + \text{cosine M}\,(\varrho'-\pi') + \alpha\,(\varrho-\pi)^2 + \beta\,(\varrho'-\pi')^2 + +$.

The moon's parallax being greater than her refraction, the second correction becomes negative; and when one of the angles is obtuse, its cosine takes the opposite sign. The fundamental formula of all approximatory methods is, therefore,

$$\text{true distance} = \mathrm{D} + \left(\frac{\text{sine } h - \sin \mathrm{H} \cos \mathrm{D}}{\text{cosin H} \sin \mathrm{D}}\right)(\varrho-\pi)$$

$$+ \left(\frac{\sin \mathrm{H} - \sin h \cos \mathrm{D}}{\text{cosin } h \sin \mathrm{D}}\right)(\varrho-\pi) + +,$$

where ϱ and π refer to that altitude h or H which stands first and by itself in the parenthesis. Lions obtained by executing the division,

$\mathrm{D} +$ (sin h secant H cosecant D $-$ tang H cotang D) $(\varrho-\pi)$ + (sin H secant h cosecant D $-$ tang h cotang D) $(\varrho-\pi)$.

By calling $\left(\dfrac{\sin h - \sin \mathrm{H} \text{ cosin D}}{\text{cosin H} \sin \mathrm{D}}\right)(\varrho-\pi)$

$$\left(1 - 1 + \frac{\sin h - \sin \mathrm{H} \cos \mathrm{D}}{\text{cosin H} \sin \mathrm{D}}\right)(\varrho-\pi)$$

$$= \left(\frac{1 - 2 \text{ cosin } \frac{1}{2}(\mathrm{D}+\mathrm{H}+h) \sin \frac{1}{2}(\mathrm{D}+\mathrm{H}-h)}{\text{cosin H} . \sin \mathrm{D}}\right)(\varrho-\pi),$$

we obtain the one part of Mendoza y Rio's approximate formula, to which the other is analogous.

By separating in Lions's method the moon's parallax from the refraction, we obtain the thence proceeding correction of the distance = hor. par. $\left[\frac{\sin ☾\text{'s alt.}}{\text{tang D}} - \frac{\sin \odot\text{'s alt.}}{\sin \text{D}}\right]$, which is identical in Elford's, Thomson's, and Lynn's tables, and is found graphically by Kelly and Norie in his linear tables, and by Thomson with his lunar scale; and it is only the *manner* of allowing for the refraction and sun's parallax wherein the above authors differ, all of whom, however, with the exception of Lynn, have erred in using in the calculation the apparent altitude in lieu of the altitude corrected for refraction. On the same account the usual tables giving the moon's correction by inspection are erroneously computed. By approximate methods are not to be understood methods that admit of less accuracy, but such ones that approach the truth by a series of which the last terms vanish, or, which answers the same purpose, by successively substituting in the calculation terms found by a former approximation. The approximative methods have, particularly to seamen, that advantage above the direct ones, that the trigonometrical calculation need only to be executed to the nearest minute; and as the corrections of the distance never can exceed those of the altitudes, and as their amount, as well as sign, may nearly be estimated from the places which the observed bodies occupy in the heavens, essential errors may easily be avoided; alterations may be made in the refraction and parallax without rendering it necessary to resume the calculation; and, moreover, they afford an easy mode of reducing this refraction to the points of the limbs brought into contact when observing their distance, as shall be shown by the following lines: we will designate by

D, the apparent distance of centres;

d, the distance of the points of contact or observed distance of limbs;

H, the greater, h the less, apparent altitudes of centres;

H$'$, the greater, h' the less, apparent altitudes of the points of contact;

r, the semidiameter parallel to the horizon (augmented if the moon's) of the upper body;

$\varrho - \pi$, the correction of the upper altitude;

$\varrho - \pi'$, the correction of the lesser altitude;

θ, the true distance of the limbs, to which the equatorial horizontal semidiameters must be added to obtain the true distance of the centres.

Let A be the distance of the middle of the apparent distance of the centres, and A$'$ the distance of the middle of the

apparent distance of the limbs from the highest point in the distance—or its prolongation, that is, from that point in a great circle drawn through sun and moon, where the effects of parallax and refraction in distance is 0,—then is

$$\text{tang } A = \frac{\text{tang } \frac{1}{2}(H-h)}{\text{tang } \frac{1}{2}(H+h) \,.\, \text{tang } \frac{1}{2} D} \text{ and } A' = \tfrac{1}{2} d + r + A - \tfrac{1}{2} D,$$

whence we obtain the apparent altitudes of the points of contact,

$$\sin H' = \frac{\cos(\frac{1}{2} d - A') \sin H}{\cos(\frac{1}{2} D - A)} \text{ and } \sin h' = \frac{\cos(\frac{1}{2} d + A') \cos h}{\cos(\frac{1}{2} D + A)},$$

which can also be found by
$H' - H = r.$ tang H tang $(\frac{1}{2} D - A)$ and $h' - h = r'$ tang h. tang $(\frac{1}{2} D + A)$ and adding $H' - H$ and $h' - h$ to the apparent altitudes of the centre above the sensible horizon and subtracting from the sun the change of refraction from H' to H and h' to h. But if $A > \frac{1}{2} D$, $H' - H$ is to be subtracted from the greater altitude, and the change of declination to be added thereto. The same is to be observed when the distance of the moon's remote limb from a star is observed. It being, however, not so much an error in the altitude as an error in the refraction which materially affects the calculation, and this refraction not being sensibly altered by a few seconds of change of altitude, the change produced by a change of refraction in the altitude may safely be neglected. For the apparent altitudes of the points of contact of the sun and moon above the horizon, compute strictly the refraction ϱ and ϱ' with regard to barometer and thermometer, and add the sums of these refractions in altitude to the apparent distance of their limbs. From the same apparent altitudes of the points of contact above the sensible horizon, find, by applying thereto the above-stated reduction, the altitudes H' and h' with respect to the true zenith, and deduct from each the corresponding refraction ρ found before, and compute for the rest the parallax in altitude π and π', and subtract their sums $\pi + \pi'$ from the observed distance augmented by the refractions, and call the rest $d + \varrho + \varrho' - \pi - \pi' = \delta$.

Find also for each altitude the corrections $\varrho - \pi$ and $\varrho' - \pi'$; then is

$$\theta = \delta - \frac{\cos(H' + \frac{1}{2} d - A').(\varrho - \pi)}{\cos H' \cos(\frac{1}{2} d - A')} - \frac{\cos(h' + \frac{1}{2} d + A').(\varrho' - \pi')}{\cos h' \cos(\frac{1}{2} d + A\ )}$$

$$+ \frac{\cos(H' + \frac{1}{2} d - A') \cos[H' - (\frac{1}{2} d - A')](\varrho - \pi^2) . \sin 1''}{2 \text{ tang } d \cos^2 H' \cos^2(\frac{1}{2} d - A')}$$

$$+ \frac{\cos(h' + \frac{1}{2} d + A) \cos[h' - (\frac{1}{2} d + A')](\varrho' - \pi')^2 . \sin 1''}{2 \text{ tang } d \cos^2 h' \cos^2(\frac{1}{2} d - A')} - - + +$$

[To be continued.]

C. Rumker.

XXIX. *Observations relative to the Structure and Origin of the Diamond.* By Sir David Brewster, *K.G.H. LL.D. F.R.S. &c.**

IN the year 1820 I communicated to the Royal Society of Edinburgh an account of a very singular fact relative to the structure of the diamond, and I added to this communication some conjectures respecting the origin of this remarkable gem. As these conjectures have been referred to by some late and able writers on the diamond mines of India without sufficiently separating the fact from the conjectures, and as I consider the structure which I discovered around the cavities in this mineral as a leading fact in the natural history of this gem, I have been induced to re-examine it with care, and to make a drawing of the phænomena which it presents.

In order to bring all the facts into one view, I shall make no apology for quoting my original observations.

"Had the diamond not been placed at the head of the mineral kingdom, from its unrivalled lustre and high value as an ornamental gem, it would have attained the same distinction from its great utility in the arts. Separated from all other gems by its remarkable refractive power, and from all mineral substances by its extreme hardness, its chemical composition, and its locality in the crust of the earth, it has always been regarded as an anomalous substance which set even speculation at defiance.

"When Sir Isaac Newton compared the refractive power of several bodies, he remarked that amber and the diamond had a refractive power three times greater in respect of their densities than several other substances, and he conjectured that the diamond was probably an unctuous substance coagulated. This relation between the inflammability of bodies and their absolute refractive power I had an opportunity of confirming and extending by ascertaining that sulphur and phosphorus exceed even the diamond in absolute refractive power, and that these three simple inflammable bodies stood at the head of all other solid and fluid substances in their absolute action upon light.

"In this arrangement, amber stood next to diamond; and as both these substances had a similar locality, and had also carbon for their base, it became of some importance to discover that their general polarizing structure was the same. The analogy, however, to which I wish to direct the attention of the Society is founded on the existence of small portions of

* From the Transactions of the Geological Society, N.S., vol. iii. p. 455. See Lond. and Edinb. Phil. Mag., vol. iii. p. 220.

air within both substances, the expansive force of which has communicated a polarizing structure to the parts in immediate contact with the air. This structure is displayed in four sectors of polarized light encircling the globule of air, and can be produced artificially either in glass or in gelatinous masses by a compressing force propagated circularly from a point. It is obvious that such an effect cannot arise from any mode of crystallization; and if any proof of this were necessary, it might be sufficient to state that I have never observed the slightest trace of it in more than 200 mineral substances which I have examined, nor in any of the artificial salts from aqueous solutions. It can, therefore, arise only from the expansive force exerted by the included air in the diamond and the amber, when they were in such a soft state as to be susceptible of compression from so small a force. That this compressible state of the diamond could not arise from the action of heat is manifest from the nature and recent formation of the soil in which it is found; that it could not exist in a mass formed by aqueous deposition is still more obvious; and hence we are led to the conclusion rendered probable by other analogies, that the diamond originates, like amber, from the consolidation of, perhaps, vegetable matter, which gradually acquires a crystalline form by the influence of time, and the slow action of corpuscular forces.

"As the preceding results were obtained from flat diamonds, which did not seem to have been regularly crystallized, I was anxious to detect the same structure in those which had a regular crystalline form. With this view I examined several of the diamonds in Mr. Allan's collection, and was fortunate enough not only to detect in a perfect octohedral crystal the same structure which I had observed in the flat specimens, but also an air-bubble of considerable size, which had produced by its expansion the polarizing structure already described."

Since these observations were written, Dr. Voysey has shown that the matrix of the diamonds produced in Southern India is the sandstone breccia of the clayslate formation; and Captain Franklin has found that in Bundel Kund the rocky matrix of the diamond is situated in sandstone which he imagines to be the same as the new red sandstone of England, that there is at least 400 feet of that rock below the lowest diamond beds, and that there are strong indications of coal underlying the whole mass. The following are Captain Franklin's observations on the origin of this mineral:

"There is another circumstance to which I must advert, but I do so with diffidence, and under a hope that it will be

considered merely conjectural. Dr. Brewster supposes the diamond to have originated like amber, perhaps from the consolidation of vegetable matter, and that it gradually acquired its crystalline form by the influence of time, and the slow action of corpuscular forces. The late Dr. Voysey adverted to this opinion in his account of the diamond mines of Southern India; and on the occasion of publishing an abstract of that paper in his Journal of Science, Dr. Brewster observed that he saw no reason to alter his opinion. Now, as the rock matrix of the diamond of *Panna* appears, in some respects, though not altogether, to resemble that of *Banganpilli* in Southern India, there would seem to be little chance of any conjecture being useful; still, however, as every opinion regarding the origin of this fine mineral is as yet theoretical, I will not withhold what occurred to me on this subject, though I again repeat that I offer it with great diffidence. The theory of Sir James Hall on the consolidation of strata frequently recurred to me when examining the sandstone in which the diamond is found: I thought that I could discern much in favour of it, and particularly in the gradual changes of its nature from the lower to the upper strata. Now, if the principle of this theory is admitted to be correct, and applicable universally, it follows of course that it must be applied here; and then it may be questioned, how the diamond was preserved under that degree of heat which must have been necessary to form its matrix the gritstone? In answer to this objection, I suggest that the circumstance of calc spar occurring in trap rocks is somewhat analogous; and if it is admitted that compression under the weight of strata and a superincumbent ocean had the effect of resisting the expansion of its carbonic acid, and constraining it to continue in combination with lime, might not the same principle be reasonably enough applied to account for the preservation and detention of the elements of the diamond in the gritstone? And, again, should it be further shown that crystals, such as those with which we are familiar in nature, may be produced by slow cooling, or other processes, according to the above theory, may we not look to it also to account for the crystallization of the gem?

"This conjecture rests upon the truth or fallacy of Sir James Hall's theory, or on a modification of it; and when this theory is considered as the result of long and patient experiment, and the high reputation of its author is taken into account, it will require something more than limited observation or ordinary ability to answer its objections; my part, however, is merely the suggestion of a traveller, and I there-

fore conclude my paper by expressing a hope that this important mineral may meet with more able investigation."

This discovery of a new matrix of the diamond takes away the foundations of the argument from which I concluded "that the compressible state of the diamond could not arise from heat," for it is possible that the rocky matrix in which it was found had an igneous origin; and Captain Franklin's supposition that it might be fused under compression, is quite conceivable.

But, though I admit the possibility of the diamond having been in a state of igneous fusion, I consider it highly improbable that it was so. In the laborious examination, which I carried on for several years, of the cavities in topaz, quartz, amethyst, chrysoberyl, &c., and in salts formed from aqueous solutions, I had occasion to observe the condition of many thousands of cavities, and in no one case, neither in crystals which exist in rocks known to be of igneous origin, nor in crystals artificially formed, have I been able to discover a single cavity in which the expansible fluid which it contained had compressed the surrounding mass, and communicated to it the polarizing structure existing around the cavities in the diamond.

Now, in glass which is known to have been in a soft state, and in amber, which is generally allowed to be an indurated gum, I have discovered cavities similar to those in the diamond, and surrounded by the same polarizing structure; a structure which could only be produced by a compressing force emanating from these cavities.

As I am desirous that mineralogists should thoroughly understand the nature of this structure, I have made two drawings of the diamond Laske which contains the cavities under consideration.

Fig. 1.

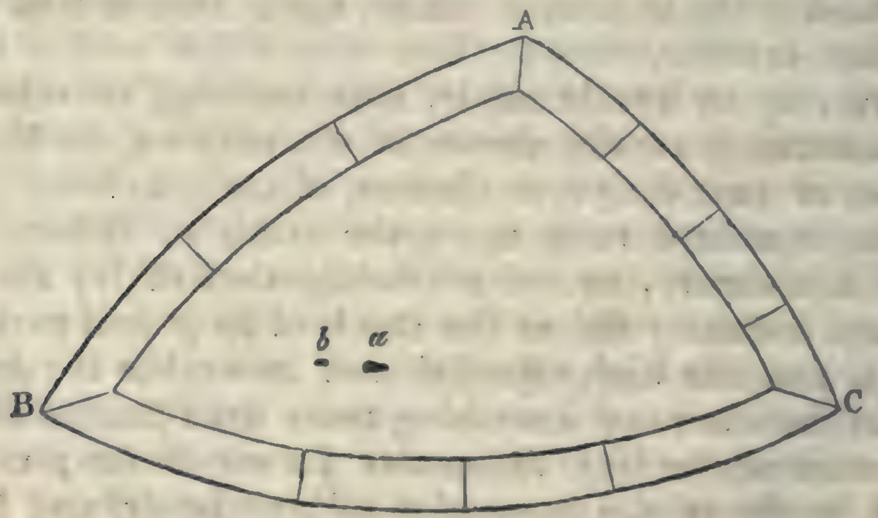


Fig. 2.

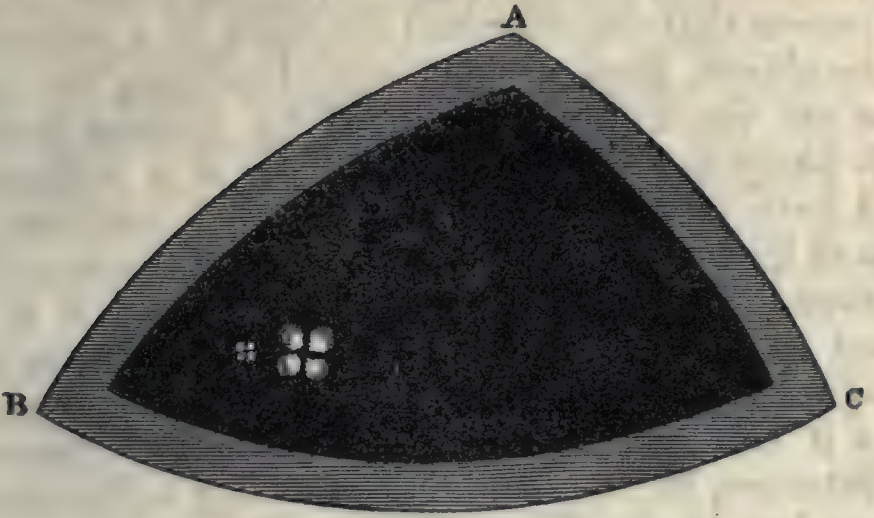


Fig. 1. represents the diamond considerably magnified. At *a* and *b* are seen two minute cavities, which appear perfectly black, as if they were filled with opake matter. This blackness, however, arises from the high refraction which takes place at the concave surfaces of the cavity, as may be proved by the application of a microscope, which exhibits a minute pencil of light transmitted through them. Fig. 2. shows the four luminous sectors around each cavity, as exhibited by the agency of polarized light. When a plate of sulphate of lime which polarizes a blue tint of the second order of colours in Newton's scale is placed across these sectors, so as to have its axis coincident with the radii of two of the luminous sectors opposite to each other, and perpendicular to the radii of the other two sectors, its blue tint of the second order is depressed, by that which is polarized by the sectors, to a red of the first order in the sectors whose radii are coincident with the axis of the sulphate of lime, and raised to a whitish yellow of the second order in the other two sectors. Hence, it follows that the character of the polarization in the sectors is negative, like that of calcareous spar, and that it has been produced by a compressing force acting outwards from the cavities.

I have, in my former paper, supposed that the compressing force was the expansive power of air included in the cavity; but this, of course, is a conjecture, though it seems quite certain that it must have been a gaseous body. That it was not a fluid is obvious, from there being no fluid in the cavities. This was certainly the case in the cavities in amber and glass; but it is possible that a fluid of very low refractive power may exist in the diamond cavities without my being able to see it, on account of the high refractive power of the gem. If this should be the case, however, it will not be difficult to observe it in larger cavities, if they should ever be discovered.

The existence of a compressed structure round the cavities clearly proves that the diamond has been in a soft state; but it may be shown, from various considerations, that this softness was not the softness produced by igneous fusion, and that it is likely to have been the softness of a semi-indurated gum. I have already stated that no such cavities exist in minerals of igneous origin; a fact which entitles us to separate the diamond from that class of crystals; and it is equally important to observe that its polarizing structure, which I have studied with peculiar care in a great variety of specimens, connects it closely with amber and indurated gum. From such substances, indeed, it differs in having a distinct crystalline form; but in the mineral resin called mellite we have an equally distinct crystalline form, though there can be little doubt, both from its composition and its locality, that it derives its origin from the vegetable kingdom.

XXX. *On the Ancient and Modern Formation of Deltas in the Persian Gulf by the Euphrates and Tigris, in answer to* Mr. Beke. *By* W. G. Carter, *Esq.*

[Continued from p. 202, and concluded.]

SINCE writing the foregoing remarks, I have seen Mr. Beke's paper on what is termed the geological evidence of the advance of the land at the head of the Persian Gulf, which commences by bringing again into notice the single passage of the historical, in which Pliny, after mentioning three different admeasurements from the gulf to Charax, a port lying near the course by the Euphrates, to Babylon,—the 1st, and shortest, made in the time of Alexander the Great; the 2nd, and longer, furnished by Juba; and the 3rd, and longest, being of Pliny's time,—the historian, very remarkably, goes on to account, as it seems, for these varying estimates by saying that the silt of the rivers had made additions to the land*. The peculiarity is, that in the preceding chapter he had given measurements of the whole distance from the gulf past Charax up to Babylon, which presented a totally different result. For the first there, is the longest, and is also made in the timeof Alexander, and the second is shorter, and yet also is furnished by Juba; and the inference there is the very natural one, that the account being thus discordant, he had not been able to determine the distance†.

* Pliny, *Hist. Nat.*, lib. vi. cap. 27.

† Errata in former part, last Number:—p. 200, *note, for* "Bosra, 12 miles below Khorna," *read* 42; p. 198, *note, for* "35 miles" *read* 37; and p. 199, *for* have *read* has.

I have before noticed, that to make the theory of the great delta and its increase of 245 miles since 325 B. C., at all coincide with the entire relation given by Pliny, we must admit the extravagant hypothesis, that the rivers had since Pliny's time separated, and then again united, and added a territory of some 200 miles to its length; or if we are not to suppose these mighty changes, but (dismissing the great delta to the winds) an increase of 70 or 35 miles only is to be inferred from the measurements to Charax, even that increase, so deduced, is negatived by the fact that these distances to Charax would actually make many more miles of country at the head of the gulf than are to be found there even at the present day*. It is, then, manifest that no dependence can be placed on them. It is the more evident that Pliny could have arrived at no greater certainty in respect of them than of the further distance on to Babylon, and that his inference from the former, that the rivers had added to the land, must, if indeed authentic, be taken subject to all the uncertainty he complains of in his data for the latter. This inference, however, with the expression of great surprise which follows it that the tide did not carry away what the rivers had brought down, is again adduced as a highly important authority in the last paper, and one is astonished at the amount of evidence which this single expression is supposed to concentrate. We are told that "it proves more evidently, that the subject of the growth of land at the mouths of rivers was entirely familiar with the natural historian, who was a native of Verona," (which remains a question,) "a city at a short distance only from the shores of the Adriatic," (it is about 60 miles distant,) "... and it further demonstrates that special attention had been devoted by him to the particular changes at the head of the Persian Gulf, since he *pointedly contrasts what he conceives ought to have been the effect of the tide there, with the consequence of the absence of tides within the Mediterranean.*"

Now, as Pliny appears in the questionable passage here referred to, to express much surprise that the tide did not carry away whatever the rivers had deposited, in fact, that any ad-

* There is a strong mark in this locality, which I should before have named. Pliny (lib. vi. cap. 28.) notices a place where once was the mouth of the Euphrates, plainly pointing to that singular inlet of the sea at the south-west of the Mesopotamian delta now called the Khore Abdallah, which seems to have been one of its ancient branch streams (still, says Dr. Vincent, called in the country its mouth,) now stopped up, at its head. Here, then, is another of those nice points of identity which we are to imagine the rivers in their vagrant transmutations to have accurately replaced since he wrote.

vance had been made by the land upon the sea, the inquiry naturally arises, what "evidence," what "demonstration" did this afford, on the principles of the reply, of Pliny's deep consideration of the topic, seeing that he thus drew, on its theory, a totally wrong conclusion? But passing that, we come to the assertion that Pliny pointedly contrasts the effect of tide in the Persian Gulf with that of the want of it in the Mediterranean. On this I have merely to observe, that neither in the context of this passage, where from the terms employed we ought to find it, nor I believe elsewhere, will Pliny be found to have made any such remark, nor even any allusion to the topic of this "pointed contrast". If it be intended only as the construction put by the writer on the above passage about the Persian Gulf, combined with the opinion that Pliny was born so near the sea as the inland town of Verona, it should, under favour, so have been given. As it is, it comes with the authority and effect of the undoubted act of that great author.

And then, rather unfavourably for this evidence of Pliny's early familiarity with the rapid formations by the Po and Adige, he has, in the former part of his work, spoken particularly of the Po, and the phenomena of its embouchure in the Adriatic. He there tells us, that from the accessions obtained by its waters, it runs over, and *most persons say,* thus forms a triangular figure between the Alps and the sea, as the Nile in Egypt makes what they call Delta*. Thus, so far from stating that he had long been "entirely familiar" with the locality, and had been watching these great alluvial changes, he is quite silent on the topic, and as to the course which the channels of the river here took, does not profess even to have seen it, but observes simply, "most persons say so".

From the conjoint tenor of these two papers, I am not very certain, however, what the amount of alluvial formation at the head of the Persian Gulf may be, which is by this time contended for. Indeed, we also seem to be thrown by the reply into much the same perplexity which bewildered Pliny. In the former paper we had a construction of Nearchus's esti-

* "His se Padus miscet, ac per hæc effunditur plerisque, ut in Egypto Nilus, quod vocant Delta triquetram figuram inter Alpes atque oram maris facere proditus stad. duum M. circuitu." (Pliny, *Hist. Nat.*, lib. iii. cap. 16.) See the whole chapter on this topic. Whether we are to read with Dr. Holland a space of 2000 paces, or with others 2000 stadia, is a point not necessary here to decide. It will, I presume, scarcely be said that Pliny, in using the word Delta, did so in the modern geological sense of an area of alluvial land. He simply thus describes the figure formed by the divided streams of the river.

mate which made the distance to Babylon 206 miles, and thus the addition to the delta since Alexander the Great 245, combined with the "assuming a construction" (of the distance to Charax) "to be more correct," which, without all those metamorphoses, would bring it to worse than nothing; and now, "if we attach no importance to Pliny's express reference to the extraordinarily rapid growth of the land in the Persian Gulf," we may, from the formation of new land within the Adriatic, "at all events be permitted to calculate" this growth in the Persian Gulf to be at least from 2 to 20 miles in 2000 years, that is, at the utmost, about 24 for the same period!

The whole geological evidence now offered, reduced to a plainer form, amounts to little more than the latter inference. Mr. Beke, however, supposes the claims of the Euphrates and Tigris to a large delta to be stronger than that of the Italian rivers, from the far greater extent of country through which the former and their tributaries sweep. But any such rule must, I apprehend, be received with very large qualifications. In the instance cited, certainly the fact lies exactly the other way. The great source of the silt of rivers is the high grounds, from which the looser matters, set free by rivulets and other causes, descend to the lowlands—the higher of course the greater the quantity,—and that again increased by the momentum it gains in its headlong passage to the plains. But what lowlands can be placed more favourably for an increase to their level by such an agency, than those about the shallow head of the almost tideless Adriatic, little removed from the end of a declivity from such a storehouse of detritus as the Alps?

The source whence the evidence is taken, and the authority adduced, is the able work of Mr. Lyell. But we are immediately there taught the utter unfitness of the comparison. "The Adriatic," (says Mr. Lyell,) "presents a great combination of circumstances favourable to the rapid formation of deltas*;" and yet the 20 miles at Adria, the maximum of this increase, is in great measure attributable, not to geological causes, but to mere human labour, for again we find "that since the system of embankment became general, the rate of encroachment of the new land on the Adriatic is said to have been greatly accelerated." M. Prony†, whose investigations seem to have led to this knowledge, says that in the 12th century, before, by the same ordinary means, a passage had been opened at the north bank of the Po, Adria was distant from the sea but 9000 or 10,000 metres, in 1600 its distance was

* Lyell's Geology, first edit., vol. i. p. 235.

† De la Beche's Geolcgical Manual, third edit., p. 70.

18,500, and now it is 32,000 or 33,000. In this way it has more than tripled in four centuries: and this is the spot which is to form a rule for that before us*.

Then, again, the Mesopotamian rivers bend their course through a locality widely different from the Italian, the Euphrates more particularly. It passes for 700 or 800 miles over a level with much sandy desert, and carries its lingering stream at a rate which will often glide over mud without disturbing it†,

* Going back to a much earlier period, we shall, perhaps, find still less reason to view either Adria or Spina as any sure landmarks for these great fluviatile encroachments. Strabo (lib. v. 214.), Livy (lib. v. cap. 33.), Justin, (xx. 1.), and all the best authorities state, that the Adriatic took its name from Adria. But the gulf bore that title at a period of from 500 to 600 years B.C., for Scylax mentions the sea of Adria, if not the city. The latter was probably of a date very ancient even to him. Yet we find from Justin, about seven centuries later, that it was still "mari proxima". (*Ibid.*) The unaided labours of the river had thus made no appreciable progress with its delta, in a period probably very far more than that in which we have seen that it has since tripled it. Ravenna, Strabo (*ibid.*) understood, was built by the Thessalians in the marshes. So Zosimus (lib. v. cap. 27). It is supposed by Rubeus (*Raven. Hist. incip.*), Amati (*Dissert. Rubic*), and other learned Italians, to be one of their settlements noticed by Halicarnassus (lib. i. cap. 16.) as made in this part long before the Trojan war. From what Jornandes about A.D. 552 (*De Reb. Get. Linden.*, p. 109,) adds to his account from Dion, it seems to have been as little in the waters then, as any visitor to the spot will now find it. Bernard Justinian (*De Orig. Urb. Venet.*, lib. i. cap. 6.), a learned Venetian of the 15th century, tells us it was then more than three miles, about its present distance, from the sea, and that the efforts of five Roman emperors had been employed to accomplish the filling up of its canals and marsh. The sea, except by the labours of man, has surely been little intruded on here. Yet Butrium, a very ancient, and supposed by Cellarius, Spretus, Amati, and others to be also a Thessalian, town, seems, from what Strabo says of it, to have been founded by the people of Ravenna, yet in the time of Pliny (iii. 15.) it was "nec procul a mari". Of Spina we hear much, Strabo stating φασι it was once situated on the coast, but was then 90 stadia inland. But "φασι (says Cellarius) dicunt homines non probunt documentis". Halicarnassus (i. 16.) says only that a city was built at the Spinetic mouth of the Padus. Taking this to be Spina, he probably means no more than the site mentioned by Scylax, who notices the distance across Italy *from sea to sea, from city to city*, and that the Greek city on the river was 20 stad. from the Adriatic. That this was the original site of Spina seems far from improbable, and Cluverius, Vossius, Gronovius seem all to agree that it was the city, Scylax thus notices. Filiasi, Amati, Frissi, and other Italian writers are much divided about its site. Now, bearing in mind that Ravenna, Butrium, and Spina were probably all of nearly the same high antiquity, and, with Adria, near to each other, and more or less subject to the operation of the same causes of change, and connecting with this, and with what we have learned from M. Prony, the great extent of marshy debateable ground between-land and water formerly existing in this part, and which might be reckoned as either, we may, perhaps, view the storied amount of geological change here as much more imposing than the true.

† De la Beche, Geol. Manual, p. 112.

making no large gatherings of buoyant matter to mingle with its waters like the fertilizing Nile*, but whenever its current is strong enough to lift earthy substances from its channel, it rather takes up comminuted granite†, or other heavy debris, which sinks when the impetus that upheld it fails‡. If to this we add, that in ancient times it became at length an extremely slender stream at its termination§, we may conclude the Euphrates to have had, within the historic era, a very moderate share in the uprearing of any delta.

In this reference to Mr. Lyell's work for the aids of analogy it was surely overlooked, that the whole joint delta formed by the mighty rivers whose waters are set free at the mouths of the Ganges and Burrampooter, is there said‖ to be of the length of 200 miles only, yet "so great is the quantity of mud and sand poured by the Ganges into the sea, in the flood season, that the sea only recovers its transparency at the distance of 60 miles from the coast, and even islands are formed in its channel in a period far short of a man's life, many miles long; some of the islands there rivalling in size the Isle of Wight." What proportion of this 200 (or 220 miles) in any fair judgement on the two localities, can we be required to add to the 50 miles of admitted delta at the head of the Persian Gulf?

But from the infinite varieties of level, soil and other local causes operating to produce and transmit the matter abraded, from the higher to the lower lands, over thousands of square miles of country, it is manifest that no such criterion applies, and I reluctantly turn from the plainer facts adduced in proof of this theory being unfounded, to deal with topics in their nature and application of a less determinate character.

I must, however, notice that "it is beyond the scope of the present paper to institute an inquiry into what may have been the direction of the coast line, when the voyage of Nearchus was undertaken." Yet it determines again that the Euphrates and Tigris had once separate outlets, and proceeds to a lengthened enumeration of investigations impliedly necessary to the present discussion¶. But it was surely forgotten, that if not within the scope of that paper, both the inquiry and the decision were within the scope of the last, for there we find "the distance from Babylon to the sea, by following the

* Pliny, *Hist. Nat.*, lib. xviii. cap. 17.

† Col. Chesney's Evidence in Report.

‡ Buckingham's Travels in Mesopotamia, ch. ii.

§ Arrian, *Exped. Alex.*, lib. vii. cap. 7.

‖ Lyell's Geology, p. 243.

¶ In the last paper we read "of a portion of the Euphrates finding a partial course through the less obstructed channels of the Tigris, and of the consequently easier and more rapid victory," &c. But it is forgotten

course of the Euphrates in that navigator's (Nearchus's) time, was only 206½ miles, whilst in the present day it is as great as about 400, it seems to me we have no alternative but to attribute the difference between these two measurements to the gain of the land upon the sea during the intervening period of 2160 years." Surely 206½ miles from a town on any road or river is plain enough. Nearchus then sailed on the gulf up to about the present town of Simauvu, where he found Diridotis, and about the present town of Duffas on the Tigris*, Alexander the Great entered its mouths. We have now merely to explain his sailing down the Euleus from Susa, the navigation across the canal, and the localities of these narrations. Though, indeed, our labours do not even terminate here, for "the possibility has been hinted to Col. Chesney, that the actual site of Babylon may be some 30 or 40 miles north-west of Hillah," a discovery which would of course require the shifting about and readjustment (if indeed the matter has been thought of) of the historical corresponding landmarks. But it is now, I believe, becoming evident, that the incongruity of all this with the plain facts of history, is beginning to be felt. Such a theory may appear feasible enough while kept within a circle of eloquent generalities; drawn thence for a closer view and application to circumstances, we find it not formed for the occasion.

Temple Chambers, July 22, 1835. W. G. CARTER.

XXXI. *Proposed Method for inferring the Dew-point from the Indications of the Wet-bulb Hygrometer.* By HENRY HUDSON, *M.D., M.R.I.A.*†

AS the expansion of air by heat is uniform, and equal to $\frac{1}{480}$th of its volume (at 32°) for each degree above that temperature, the relative volumes at different temperatures will (*cæteris paribus*) be proportional to $\overline{448 + t,}$ and, of course, the relative *densities* will be inversely as $\overline{448 + t}$: hence putting f' for the elastic force of (or pressure on) air, and t' for any temperature, we have density at 212° (under 30 pressure): density at 212° under f' :: 30 : f'; also density at 212°

that the channels of the Tigris are clearer on account of the greater rapidity of their current down the declivity towards the Euphrates, whose waters could not run in such channels without running up a slope. I have before pointed out, that this declivity distinctly negatives the joint delta of two long separated streams.

* See Col. Chesney's Map in Report.

† Communicated by the Author.

under f' : density at t under f' : : $\overline{448+t}$: 660; ∵ density under 30 at 212°: density at t' under f' : : $\overline{448+t} \times 30 : 660 f'$: : $\overline{448+t} : 22 f'$.

Assuming now the atomic theory of volumes, the number of particles will be the same in a given volume of air at 212° under 30, and of vapour at 212° with same elasticity. Also air at t under f' contains (in given volume) same number of atoms as vapour formed at t with elasticity $=f'$. Hence the relative densities of vapour at different temperatures will be given by the above proportion, or, taking density of air at 212° under 30 as = 1, density of vapour at 212° under 30 = ·625 (Despretz), therefore *density of vapour at t*

$$= \frac{\cdot 625 \times 22 f'}{448+t} = \frac{13\cdot 75 \times f'}{448+t} \quad \text{(A.)}$$ (f' being tension of vapour at t)*.

Now, taking latent heat of vapour formed at 212° = 956 (Despretz), and considering the sum of the latent and thermometric heat to be a constant quantity, we have the latent heat of vapour at any other temperature $t = 956 + 212 - t = 1168 - t$. But the quantity of heat requisite for converting into vapour all the moisture that can exist at the temperature t is found by compounding the ratios of the quantities of moisture with the ratios of the latent heats at different temperatures: thus,

Latent heat at 212°: latent heat at t : : 956 : $1168-t$, and *quantity of moisture* at 212°: quantity at t : : ·625 : $\frac{\cdot 625 \times 22 f}{448+t}$; therefore, putting Q and Q′ for the *quantities of heat* requisite to convert into vapour all the moisture that can exist (in a given volume) at 212° and at t, we have Q (at 212) : Q′ (at t) : : 956 : $\frac{\overline{1168-t} \times 22 f}{448+t}$ (B.).

* This formula may be proved otherwise; thus, putting D for density of vapour of saturation at t with elasticity $=f'$; D′ for density of *same* vapour heated to 212° and preserving same elasticity; and D″ for density of vapour at 212° under 30 pressure, we have

$$D : D' :: 660 : \overline{448+t}$$
and
$$D' : D'' :: f' : 30$$
$$\therefore D : D'' :: 22 f' : \overline{448+t} \text{ and } D = \frac{D'' \times 22 f'}{448+t}.$$

My object in deducing it in the other manner is to show that the fact of the "maximum tension of vapour being wholly independent of the pressure, and dependent solely on the temperature," is a necessary consequence of the above view of the atomic theory.

Now, in the wet-bulb hygrometer, when liquid becomes stationary, the temperature of wet ball gives the temperature of vapour carried off, and also of the air which carries away this moisture, otherwise the instrument could not remain stationary. Hence the heat lost by air in sinking from its previous temperature to that of wet ball, gives the latent heat of the vapour formed. Now, as no increase of the *velocity* with which air approaches ball makes any difference in the temperature of hygrometer, I must conclude that the air thus chilled in causing evaporation carries off *all* the vapour that can exist in it *at this reduced temperature*; and hence (if the air be *perfectly* dry) that the *depression* of temperature of hygrometer gives the measure of the quantity of heat requisite for the conversion "into vapour of *all the moisture that can exist* in a given space at the temperature of the wet ball," or, in fact, that, (in perfectly dry air at different temperatures, the depressions of temperature of wet ball ought to be *proportional* to the quantity $Q' = \left(\frac{\overline{1168 - t \times 22 f'}}{448 + t}\right)$ for the temperature of hygrometer in each case.

If air be not dry, of course hygrometer will be cooled less in proportion to the previous *dampness* of air. Thus if hygrometer falls in dry air from $\underline{t + V}$ to t, and in moist air from $\underline{t + d}$ to t; then $V : V - d$: : the *moisture of saturation* at t : "the quantity of moisture *existing in this air*," or, in fact, to the "*moisture of saturation* at the *dew-point*." Hence, if the value (in degrees of the thermometer) of *any one* of the *proportional numbers* representing the relative fall in dry air be found, you can at once by proportion assign the depression in dry air at any other temperature of the hygrometer; and consequently, from this, infer the dew-point from the difference of temperature of thermometer and wet ball by the proportion V (the fall in dry air) : $V - d$ (d being fall in moist air) : : m'oisture of saturation at t' (temperature of hygrometer) : m''oisture of saturation at dew-point. (See table at end.)

Suppose (for example) that, when hygrometer marks 61° in dry air, thermometer stands at 122°·124 (V being = 51°·124. Hence since the thermometric value of Q' at 61° = 51°·124, all the numbers in fourth column of table (derived from formula B) should be increased in the proportion of 25·933 : 51·124, in order that they should represent the *number of degrees* that *dry* air should cause the hygrometer to fall at the various temperatures of the latter. Now, suppose hygrometer marks 61° in moist air, while thermometer marks 100°, we have (by proportion) $V : V - d$, i. e. 51·124 : 39 : : ·0146475

(moisture of saturation at 61): ·01117382 = density of vapour of saturation at 52°·408, *the dew-point* of the air in question.

To inquire into what the *actual values* (in degrees of Fahrenheit) may be of these *proportional numbers* (derived from formula B) we may adopt three different courses. We may calculate them, 1mo, From assumptions as to the capacity of air for heat, &c. &c.; 2do, From actual observation of the values of the numbers in the table (in degrees of the thermometer) when dry air makes hygrometer stand at *any* temperature; 3tio, By conjoint observations of the thermometer, wet-bulb, and dew-point of the air which has produced this reduction in temperature of hygrometer.

Thus, 1mo, Suppose that De la Roche's estimate of capacity of air for heat is correct, or that capacity (by weight) of water : capacity (by weight) of air :: 1000 : 267. Now, latent heat of *vapour of saturation* at 212° = 956; hence it should raise temperature of *same weight* of air 3580°·5 (for 267 : 1000 :: 956 : 3580·5). Hence also (*vice versâ*) *supposing capacity of air not to vary with the temperature*, the latent heat required by the "vapour of saturation" at 212°, ought, in its vaporization, to reduce temperature of dry air (in equal weights) through 3580°·5 (down to 212°). But supposing air to carry off its saturating quantity of moisture, the weight of air will be to weight of vapour :: 1000 : 625; ∴ the reduction of temperature should be less in that proportion which would give $\left(\frac{3580 \cdot 5 \times 625}{1000} = \right)$ 2237° for the *actual reduction* on these suppositions. I shall not, however, dwell further on this method, nor the corrections it would require, as I place no reliance on the truth of the requisite assumptions *.

2do. Since (B.) gives the *quantities of heat* which dry air must lose when (in sinking to the temperature of hygrometer) it has become saturated with moisture, it follows that if we know the number of degrees through which this air falls at any one temperature of hygrometer, we can find (by pro-

* If Dr. Apjohn's assumed data should be admitted, his formula corrected would become

$$\frac{\overline{1179 - t} \times 39 \cdot 6382}{\overline{448 + t} \times (\cdot 0223\, p + \cdot 3312)} \times f = \text{weight of moisture of saturation (at } t)$$

of that number of cubic inches of air which in falling *one degree* vapourize *one grain* of water; therefore this weight in grains is the same as the *number* of degrees *dry* air must fall when hygrometer stands at this temperature in it, *i. e.* it is the number V in my formula, and (if correct) might be used in the way I have pointed out to infer the dew-point.

portion) what number of degrees dry air should fall at any other temperature of the hygrometer on the *supposition* that air in sinking a given number of degrees (at those different temperatures) gives out the same quantity of heat in each case, *from a given volume.* If this be not so, we must allow for it in the manner to be hereafter mentioned. To try the amount of effect of dry air at different temperatures on wet-bulb hygrometer, I would propose to pass air (well dried by muriate of lime) from a gasometer through a tube which could be heated to different degrees at pleasure, and in the *continuation* of this tube (*made of glass*) to suspend a thermometer and wet-ball hygrometer, so as to be equally exposed to current of air, and note the temperature of each (in a tabular form) as the temperature of air varied. The current should be rapid, and the source of heat large, so that temperature should alter slowly. There should be various precautions adopted, which I shall not dwell on here.

3tio. In order to ascertain the value (in degrees of Fahrenheit) of the *proportional numbers,* we may evidently use the formula "V : V − *d* : : moisture of saturation at temperature of hygrometer : moisture of saturation at dew-point," by conjoint observations of both hygrometer and dew-point. Thus, suppose (for example) that thermometer marks 100° and hygrometer 61°, and dew-point found by observation = 52°·408, then (by inverting former formula) we have "moisture of saturation at 61°; moisture of saturation at 52°·408 : moisture of saturation at 61° :: (100 − 61) 39 : 51°·124 = V, the fall in dry air corresponding to temperature 61° of hygrometer.

Having tried Daniel's hygrometer (with this view), I doubt whether we can attain (through *this or similar* instruments) a knowledge of the actual dew-point without being *liable* to an error of, perhaps, a degree or more, at the ordinary temperatures of the dew-point in these countries. It must be evident from the very small *differences* of the saturating weights of moisture (in a cubic foot of air) at the ordinary temperatures of the dew-point, where the temperature alters one degree, that the amount deposited in a short time can hardly be perceptible to our senses. For instance, suppose the point of saturation is 58° and that temperature is reduced to 57°, the quantities of saturation (in a cubic foot) are 5·492 grains (at 58) and 5·345 grains (at 57), so that a *cubic foot* of air acting on a *given surface,* could not, under these circumstances, deposit 0·15 grains of moisture on that surface (although 1° below its point of saturation). Now, unless the *surface* be very *small* (in which case the *time* which a cubic

foot of air requires to deposit its moisture by successive contacts becomes considerably increased), I doubt if our senses could appreciate such an amount of dew at all; and on the supposition of a considerable *time* being necessary for the deposition of a sensible amount of moisture (from air at 57°, the point of saturation being 58°), the chances that the temperature will remain steadily *even at* 57° (long enough for us to observe the process) are with such an instrument very slight. Of course, the lower the dew-point, the greater the error from this cause is likely to be. This mode of inquiring into the value of the numbers (given by formula B.) has nevertheless its advantages, as whenever the hygrometer stands at the SAME point, observation of the dew-point gives us a *new* value of V (the depression in dry air) at that temperature of hygrometer; and by taking a *mean* of various values, and making allowances for the probable amount of error in different cases, we may *approximate* to the true value of V.

The best method of ascertaining the value of V on this principle is obviously similar to that which I have pointed out (p. 260.) for ascertaining V directly, and is much more easily executed, as the thermometer and wet-ball need only be exposed equally to a current of the *atmospheric* air (previously heated), for in this case the mixture of the external air with the heated air can produce no error, as they have the same dew-point. The experiment would be best tried when the atmosphere was perfectly damp (*i. e.* when temperature of thermometer and wet-ball were the same), as then the knowledge of the dew-point would be perfect. If not so, however, the dew-point should be ascertained by Dalton's method, and then expose thermometer and hygrometer to same air heated, marking their cotemporaneous indications in a table. Then calling t temperature of thermometer, and t' temperature of hygrometer; also m' = moisture of saturation at t', and m'' = moisture of saturation at t'' (the dew-point), we have

$V = \overline{t-t} \times \dfrac{m'}{m'-m''}$ for the temperature of the hygrometer in each case*.

Now, supposing the atmospheric pressure to be = 30 at the time, and to continue the same during the experiment, and

* We might, perhaps, use a method somewhat similar to the one alluded to above for ascertaining the dew-point, viz. by directing a current of cold air (equally) in thermometer and wet-bulb, and marking the exact temperature at which thermometer and hygrometer became of the same temperature. This would probably, however, be of more difficult execution. It may not be improper to state here, that I communicated the views

that t'' has been accurately ascertained, it would evidently follow that the values of V thus found for different temperatures of the hygrometer should be proportional to the numbers I have given in the table (4th column), *unless* "a given volume of air of different temperatures under 30 pressure has different capacities for heat." Hence this very desirable information may be obtained conjointly with the values of V under that pressure. Also, if the same experiments be repeated under a different atmospheric pressure (say 28), we find the values of V for various temperatures of hygrometer; and if variation of pressure (the temperature being the same) makes a difference in the capacity of air* *estimated by volume*, we find the amount of that difference at the different temperatures of the hygrometer, and of course obtain another valuable addition to our knowledge. The latter point may also be elucidated in the following manner, viz. by covering the *balls* of a good differential thermometer with linen, and at known temperatures (when pressure varies) wetting one of the balls with some *volatile liquid* (spirits of turpentine, for example,) whose evaporation will not cause sufficient cold† to produce a deposition of dew (which might disturb the process by the latent heat of the vapour deposited). We may thus ascertain what effect change of density produces on the capacity of air estimated by volume, when the other circumstances of the experiment are perfectly similar.—Experiments on the depression of temperature produced by the evaporation of this and other volatile liquids, may (I need hardly add) be

contained in this paper to my friend Dr. Apjohn two or three days after he communicated his method to the Royal Irish Academy, and I have recently had the pleasure to learn from him that he has employed *both the methods* of experimenting on dry air, and on air of a known dew-point (heated artificially), with the view of testing and establishing the correctness of his own formula: this (of course), from the reasons mentioned, I am convinced they cannot do; but I look forward to his intention of publishing his experimental results with much interest, as likely to elucidate the entire subject.

* I am inclined to think that change of atmospheric pressure will not (*cæteris paribus*) affect the value of V. The *expansions* of all gases and vapours (of whatever density) at a given temperature are the *same* from *equal additions* to that temperature, and this *expansion* or increased *repulsive force* is caused, in each case, by the addition or communication of a certain quantity of *heat* (as distinguished from temperature). The simplest cause, therefore, that would account for the equality of expansion from equal temperature, would be that equal additions of *temperature* were also equal additions of *heat* in each case, or, in other words, that the capacities for heat are equal in equal volumes. I need scarcely say, that I consider experiments with sulphuric acid in a receiver (more or less exhausted) as completely fallacious.

† Æther might be used with advantage if the air were very dry.

resorted to with advantage *in the manner* already described for ascertaining V by conjoint observations of hygrometer and dew-point*. The principal recommendations to the method I have proposed are, 1st, that the assumed data (viz. the law of dilatation of gases from heat, the latent heat of steam, and the equality of the *sum* of the latent and thermometric heat at different temperatures,) are all supported by concurrent experimental evidence with very trifling differences. 2nd, That there are no calculations founded on disputed points (viz. the capacity of air for heat, nor of vapour for heat, nor on the changes of capacity of either air or vapour produced by changes of temperature and pressure). 3rd, That the method itself enables us to eliminate the effects arising from the principal causes of uncertainty, and, in making the method more accurate, furnishes valuable information on collateral subjects. 4th, If at tolerably high temperatures (where V is large) we should not be able to approximate (with certainty) within one or two degrees of its real value, that the error that will ensue in the determination of the dew-point at ordinary temperatures will be so small that it may be fairly disregarded. I have given a table in which the 1st column represents the temperatures of hygrometer, the 2nd column is Dalton's corresponding "tension of vapour," the 3rd column gives the relative densities of the vapour of saturation of a given volume at those temperatures as derived from formula (A.). The 4th column gives the *relative quantities* of heat requisite to vapourize the moisture of saturation at those different temperatures derived from formula (B.); and in the 5th column I have *attempted* to give the values of (V) the fall of hygrometer in dry air. In forming this I have taken 51°·124 as the value of V at 61, that being the *mean value* deduced from the FIVE *sets of experiments* on conjoint observation of dew-point and wet-bulb (at 61°) in the table attached to Dr. Apjohn's paper. Of course, I do not mean to press its *accuracy*; but a comparison of the observed dew-points with those calculated from it at the other temperatures of hygrometer would lead me to think it not to be very far from correct. If the plan now proposed were to be adopted, it would be desirable to *interpolate* the values (for at least each quarter of a degree) in the various columns of the table.

March 16, 1835. HENRY HUDSON.

* We can *thus* ascertain by experiments, when *temperature and pressure* are both alike, but the *humidity* of atmosphere *different*, what effect on capacity of air, estimated by volume, may arise from this cause.

P.S. Since the addition of the last page, as well as of the note on page 259, it has occurred to me that the formula $V:V-d::$ moisture of saturation at hygrometer temperature : moisture of saturation at dew-point, ought to be altered to $V:V-d::f'$ (tension of vapour at hygrometer temperature) : f'' (tension of vapour at dew-point). The former is true of a given volume of vapour cooled (with diminution of *elasticity*) without alteration of density, as in the case of vapour cooled in a tube hermetically sealed. The latter refers to the case where a given volume of vapour (cooled) *increases* in *density* (preserving the same elasticity), and is more applicable to the actual state of the atmosphere. In this respect, therefore, Dr. Apjohn's point of view was more correct. And it would appear to me that our *methods* would in this case be perfectly alike, the difference only consisting in Dr. Apjohn's obtaining his value of V from theoretical views on which I have no reliance. It would appear, however, that Dr. Apjohn does not view the matter in this light, as he has had the kindness recently to communicate to me the following proportion as derived from his views, viz. If V be the depression in dry, and d in moist air, f^{-} the elastic force of vapour at stationary temperature of hygrometer in dry, and f' in moist air, and f'' the elastic force at dew-point of the latter, then, he states that

$$V:d::f^{-}:f''+f^{-}-f',$$

instead of being, as I conceive it should be,

$$V:d::f':f'-f'';$$

in fact, his proportion would make d greater than V. Of course, the terms f' and f'' should be substituted for m' and m'' (*i. e.* the *tension* of vapour at the different temperatures for the *density* of vapour at the same temperatures,) the various places where V or t'' (the temperature of dew-point) is to be found by *proportion* in the preceding pages.

In concluding, I beg to state that I hope very soon to bring forward experiments to *prove* that *radiation* can have *no* effect on the indications of the wet-bulb hygrometer, unless surrounding bodies are of a *different* temperature from the atmosphere.

	Tension of Vapour.	Density of Vapour.	Relative Quantities of Heat necessary to produce Tension of Vapour at those Temperatures.	*Values* of the *proportional* Numbers in 4th Column, *assuming* Value at 61° 51°·124 Fahrenheit.
212°	·30	·625	956·	1884·76
70	·721	·01914	33·622	66·286
69	698	.018564	32·643	64·355
68	676	·018014	31·703	62·5026
67	·655	·017488	30·806	60·7343
66	·635	·016986	29·95	59·0465
65	·616	·01651	29·138	57·437
64	·597	·016032	28·32	55·832
63	·578	·015552	27·497	54·20975
62	·560	·015098	26·7175	52·6737
61	·542	·0146475	25·933	51·124
60	·524	·014183	25·143	49·57
59	·507	·01375	24·398	48·1
58	·490	·013315	23·648	46·622
57	·474	·012906	22·941	45·227
56	·458	·012495	22·23	43·8266
55	·443	·012110	21·565	42·5256
54	·429	·011750	20·944	41·2914
53	·415	·011390	20·32	40·06
52	·401	·011027	19·69	38·8196
51	·388	·010691	19·107	37·67
50	·375	·010350	18·521	36·5414
49	·363	·010043	17·98	35·4474
48	·351	·0097304	17·465	34·4317
47	·339	·0094165	16·89	33·299
46	·328	·0091295	16·389	32·31
45	·316	·0088134	15·836	31·22
44	·305	·008524	15·33	30·223
43	·294	·008233	14·82	29·2177
42	·283	·007941	14·3075	28·2074
41	·273	·007676	13·842	27·29
40	·263	·007410	13·374	26·36675
39	·254	·007171	12·954	25·539
38	·245	·0069315	12·532	24·7068

XXXII. *Experimental Investigation of a Formula for inferring the Dew-point from the Indications of the Wet-bulb Hygrometer.* By JAMES APJOHN, *M.D., Professor of Chemistry in the Royal College of Surgeons, Ireland.**

AT the meeting of the Academy held in November last, I was permitted to read a short memoir† on the subject of a formula, at which I had a considerable time previously arrived, for inferring the dew-point from the indications of the moist-bulb hygrometer. This formula was deduced altogether from general considerations, and, though satisfied from some hasty observations of my own that it represented facts with considerable accuracy, I was not, at the time, in possession of evidence which could be considered as establishing this important point in an unequivocal manner. The table which is subjoined to my paper undoubtedly shows that, within certain limits, my formula is in accordance with experiment; but the observed depressions in the table are, generally speaking, so small, that a formula in itself incorrect might, it must be admitted, yield results which would deviate from the observed dew-points, by quantities not exceeding the possible errors of observation. Berzelius, for example, states (*Traité de Chimie*, tom. viii. 6. 254.) that from the experiments of August, Bohnenberger, and others, it appeared that the temperature of a thermometer, with moistened bulb, was an arithmetic mean between that of the air and the dew-point; and this rule, which would make $t'' = 2\,t' - t$, though utterly erroneous, would apply to the table appended to my paper nearly as well as the formula I have deduced. The validity, therefore, of my method required to be more rigorously tested, and having been for some time engaged in experimental researches instituted with this object, which have led to interesting, and to me most satisfactory results, I am anxious to submit them with as little delay as possible to the judgement of the Academy.

The equation which, as I believe, comprehends the theory of the wet-bulb hygrometer is as follows:

$$f'' = f' - m\,d \times \frac{p}{30},$$

* Communicated by the Author; having been read before the Royal Irish Academy, on April 27, 1835.

† The substance of this memoir has since been published in the Lond. and Edinb. Phil. Mag., vol. vi. p. 182. Those who would refer back to it should make the following corrections. Page 183, line 25, for "water" read *air*. Same page, line 33, for $\left(27 \times 448\,t' \times \frac{30}{p}\right)$ substitute $\left(27 \times \overline{448 + t'} \times \frac{30}{p}\right)$. Page 184, line 2, for 37, in both places where it occurs, write 87. Page 186, line 17, for "elimination" read *dimension*.

in which f'' is the tension of steam at the dew-point, f' its tension at the temperature of the hygrometer, d the depression or difference between the temperature of the hygrometer and air, p the existing and 30 the mean pressure, and m a coefficient depending upon the specific heat of air and the caloric of elasticity of its included vapour, its arithmetical value being ·01149, or the equivalent vulgar fraction $\frac{1}{87}$. In the paper to which I have already referred corrections are given for the influence on the specific heat of air of the fluctuations of the barometer, and the moisture present in the atmosphere. These corrections are, I believe, deduced from correct principles, and should be resorted to when extreme precision is desirable. Experience, however, and a careful consideration of the subject have satisfied me that they are, generally speaking, in their effects much too insignificant to be objects of attention to the practical meteorologist.

The first and most obvious method of verification which presented itself to my mind was the comparison of my formula with recorded cotemporaneous observations on the temperature of air, that shown by a moist-bulb hygrometer, and the actual dew-point. I have, however, unfortunately been able to meet but few at all suited to my purpose. Those in which $t - t'$ is small, and this is generally the case in the few registers to which I have had access, cannot, as we have already seen, serve for deciding the value of any formula. In the First Report, indeed, of the British Association for the Promotion of Science, page 50, mention is made of a register of observations kept in the East Indies, which, as belonging to high temperatures, would necessarily exhibit great depressions, and would therefore be valuable as a standard of comparison; but I have in vain searched for the Calcutta Journal "Gleanings in Science," in which they are said to be contained. In fact, the only observations I have been able to procure adapted to my purpose, and made, apparently at least, with the necessary precision, are those adduced in the article HYGROMETRY of Sir David Brewster's Encyclopædia, and there made by the author of the article the basis of a calculation for investigating the constants of a tentative formula for connecting the indications of the wet-bulb hygrometer with the dew-point. They are but two in number, and are comprehended in the following table, in which the numbers in the first column represent the temperatures of air, those in the second the corresponding indications of the hygrometer, those in the third the depressions, those in the fourth the pressures, and those in the fifth the dew-points experimentally determined by the method of Dalton.

(1.) t	(2.) t'	(3.) d	(4.) p	(5.) t'' obs.	(6.) t'' calc.
67·2	52	15·2	29·75	35·7	35·6
56·4	49·5	6·9	30·02	39·5	42·4

The numbers in column (6.) are the dew-points calculated by my formula; and while there is an almost exact correspondence between the first and the result of experiment, the second, it will be seen, is higher than the observed temperature of deposition by nearly three degrees. There is here, however, obviously some mistake. It is impossible that with the recorded temperatures of air and hygrometer the dew-point could have been so low; and this conclusion I do not at present draw from my theoretical views, for that would be to subject myself to the imputation of arguing in a circle, but from the following observation made by me with great care on the 22nd of March.

t	t'	d	t'' obs.
56	50	6	44

Here the temperatures t and t' differ from those taken from the Encyclopædia only by about half a degree, and nevertheless the observed dew-point 44 is higher than 39·5 by 4·5 degrees. From these observations, therefore, I am, I conceive, entitled to conclude, 1st, that the series in which the depression amounts to 15°·2, being in exact accordance with my formula, lends it some degree of support; and 2ndly, that my method cannot be considered as impugned by the other series, in as much as *this* is in some particular manifestly incorrect. But it is time to enter upon the experimental tests to which I have resorted.

If air, in reference to which t, t', and t'' have been accurately noted, be raised to any elevated temperature, and the observation be repeated in the heated air as far as respects t and t', we shall have two* separate sets of observations, from which to calculate the point of deposition; and as the amount of moisture in the air is not altered by the augmentation of temperature it has experienced, both calculations, provided our formula be correct, should give precisely the same result, *i. e.* the dew-point in the first instance determined by observa-

* Any number of observations, having reference to the same dew-point, may, it is obvious, be thus obtained.

tion. Such is the principle of the test experiments which I first performed. The air was heated by urging it in a continued stream by means of a double bellows through the worm of a small still, such as are for sale in the opticians' shops, the worm-tub being filled with water of the desired temperature; and, in order to the necessary observations, in a glass tube, connected by a cork with the upper extremity of the worm, a couple of small thermometers were placed, their bulbs being separated by about a quarter of an inch, and that of the instrument occupying the higher position being invested with a tunic of muslin kept constantly moist with water. The blast was steadily maintained until the thermometers ceased to rise, and the temperature of each was then accurately noted, the eye being assisted by a lens. Tables I., II., III., and IV. exhibit the results of four distinct series of experiments thus conducted.

Table I.

February 8, 1835. 11 o'clock A.M.

	t	t'	d	p	t'' obs.	t'' calc.	Diff.
1	49·6	44·7	4·9	29·6	40	39·02	—·98
2	88·5	62·	26·5	29·6	40	39·18	—·82
3	80·5	59.	21·5	29·6	40	39·27	—·73

Table II.

February 9, 1835. 11 o'clock A.M.

	t	t'	d	p	t'' obs.	t'' calc.	Diff.
1	47·2	42·5	4·7	30·02	38	36·58	—1·42
2	76·	57·5	18·5	30·02	38	40·44	+2·44

Table III.

March 4, 1835. 11 o'clock A.M.

	t	t''	d	p	t'' obs.	t'' calc.	Diff.
1	48·3	43·	5·3	29·76	37·5	36·41	—1·09
2	96·	64·	32·	29·76	37·5	36·37	—1·13
3	91·	62·5	28·5	29·76	37·5	37·66	+1·16
4	75·	56·	18	29·76	37·5	38·26	+1·76

TABLE IV.

March 25, 1835. 11 o'clock A.M.

	t	t'	d	p	t'' obs.	t'' calc.	Diff.
1	51·3	45·5	5 8	30·7	38·5	38·61	+ ·11
2	82·	59	23	30·7	38·5	36·7	−1·8

The results exhibited in the preceding tables will, I believe, be considered by many as going far towards establishing the accuracy of my theoretical views. Although the depressions vary from 4°·7 to 28°·5, the differences between the observed dew-points and those deduced from the formula are certainly not greater than what may fairly be ascribed to unavoidable inaccuracy of observation. But for the purpose of putting this matter in a still clearer point of view, I have calculated a number of values of m, the constant of our formula, from the preceding observations. This was easily done; for as all the observations in the same table refer to air in the same hygrometrical state, each series should give the same dew-point, and the expression $f' - m\,d \times \frac{p}{30}$ must have in reference to them a constant value. $f' - m\,d \times \frac{p}{30}$ for one, must, therefore, be equal to $F' - m\,D \times \frac{p}{30}$ for any other, an equation from which we deduce $m = \frac{F' - f'}{D - d} \times \frac{30}{p}$. The application of this method gives us the following values of m.

Table 1.

	(1 & 2)	(1 & 3)	(2 & 3)
$m =$	·01155	·01185	·01075

Table 2.

	(1 & 2)
$m =$	·01489

Table 3.

	(1 & 2)	(1 & 3)	(1 & 4)	(2 & 3)	(2 & 4)	(3 & 4)
$m =$	·01137	·01187	·01309	·00825	·00976	·01045

Table 4.

(1 & 2)

$m =$ ·00967.

If the mean of all these values of m be taken, it will be found to be ·01122, or the equivalent vulgar fraction $\frac{1}{87}$, an approximation to the coefficient $\frac{1}{87}$ employed in the formula which, under all the circumstances, cannot but be considered as

remarkably close. Indeed, the difference, which is less than three in the fourth place of decimals, is so small that they may be substituted indiscriminately for each other without the occurrence, at least in ordinary cases, of sensible error. Had values of m been calculated from the comparison alone of the first series of observations in each table, with the subsequent ones, the mean, it is worthy of remark, would be ·01156, or almost exactly $\frac{1}{87}$; and as, for such observations, $F' - f'$, and $D - d$ are necessarily greatest, they are best calculated to afford correct results, since any error of experiment would obviously in their case exercise the least influence.

The next test experiments performed were suggested by the formula itself. If $f'' = f' - \frac{d}{87} \times \frac{p}{30}$, and f'' be supposed equal to 0, a condition which can only be fulfilled in perfectly dry air, $f' = \frac{d}{87} \times \frac{p}{30}$, an equation from which we deduce $d = 87 f' \times \frac{30}{p}$. Hence by determining experimentally the depression of the hygrometer in perfectly dry air, we shall be able to pronounce upon the validity of the general method under discussion.

The first attempts for determining values of d experimentally, consisted in suspending a pair of thermometers, one of which had its bulb moistened, in a close-corked bottle, the bottom of which was covered with a stratum of oil of vitriol; but this method was soon abandoned, as the depressions it afforded were, on an average, one fifth less than they should be according to the formula. In fact, the extreme depression could not be expected here, for it is obvious that the air in contact with the bulb of the moist thermometer is never perfectly dry except at the very commencement of the experiment.

The next contrivance to which I resorted was as follows: A bag of India-rubber cloth, furnished with a cap and stopcock, was inflated by a bellows, and thus connected by means of a caoutchouc collar to a glass tube traversing a cork fitted to the tubulure of the lower bottle of a Nooth's apparatus. The middle bottle of the apparatus was next filled two thirds with oil of vitriol, and the pair of thermometers last described being introduced into the axis of a small tube perforating a cork fitted to the upper opening of this bottle, a stream of air was forced by pressing on the caoutchouc bag through the oil of vitriol, and, of course, over the thermome-

ters, and as soon as the instrument with moistened bulb ceased to fall, the temperatures of both were noted.

The following Table comprehends the results of five experiments thus performed:

	t	t'	p	d obs.	d calc.	Diff.
March 11.	48·5	31·5	29·37	17·	17·4	— ·4
15.	50·5	33·5	30·00	17·	18·2	—1·2
20.	54·5	35·5	30·25	19·	19·4	— ·4
21.	57·5	37·5	30·27	20·	20·8	— ·8
22.	54·5	36·0	30·35	18·5	19·7	—1·2

Now, as in all these instances the observed depression differs from the true, this difference, though small, being always on the same side, must be ascribed either to the coefficient m being assumed too great, or to the method of experiment employed not being calculated to afford the extreme depression. That this latter was the real cause of the discrepancy I was disposed to believe from having observed that when the hygrometer in the course of an experiment became stationary, it could be made to sink a little further by pressing with great force upon the bag of air. In fact, this observation rendered it probable that the tube between the lower and middle bottle of the North did not afford sufficient air-way, and that, therefore, there was not a sufficient current from behind to propel forward, and immediately remove from contact with the moistened bulb, the air which had become saturated with its humidity. To bring this conjecture to the criterion of experiment it was obviously necessary to operate so that, while the air underwent perfect desiccation, it was at the same time made to pass over the thermometers in a strong and continuous current; and after some trials I found that both objects were secured by substituting for the Nooth a series of three Wolfe's bottles containing oil of vitriol, and connected, as in the process for preparing the water of ammonia, by glass tubes and caoutchouc collars, the bag of air being attached to a tube passing to the bottom of the first bottle, and the thermometers being placed in the axis of a tube perforating a cork inserted into one of the tubulures of the last bottle.

The experiments recorded in the following table were made with this apparatus.

	t	t'	p	d obs.	d calc.	Diff.
March 26	51	33·5	30·55	17·5	17·94	+ ·44
27	53	34·5	30·35	18·5	17·73	— ·77
28	52	34	30·21	18	17·62	— ·38
29	51	33	30·05	18	17·97	— ·03
30	52	33·4	29·75	18·6	18·37	— ·23
31	53	34·3	29·50	18·7	19·14	+ ·44
April 1	56·5	35·8	29·70	20·7	20·04	— ·66
2	58	37	29·72	21	20·88	— ·12
3	58·2	37	29·77	21.2	20·84	— ·36
4	58	37	30·03	21	20·68	— ·32
5	58	37	30·15	21	20·59	— ·41
6	59	37·5	30·25	21·5	20·88	— ·62
7	59	38	30·26	21	21·24	+ ·24
8	61	38·7	30·21	22·3	21·80	— ·50
10	58·3	37·7	30·35	20.6	20·96	+ ·36
11	58	37·5	30·45	20·5	20·75	+ ·35
12	56·3	36·5	30·30	19·8	20·12	+ ·32
13	57·5	37	30·20	20·5	20·55	+ ·05
14	57·5	37	30·15	20·5	20·59	+ ·09

Of the nineteen observations of depression in dry air registered in the preceding table, eleven are greater and eight less than the calculated results. The mean of the plus errors of the formula is ·28, and of the minus errors 4, of a degree; so that ·28 — ·40 = — ·12 of a degree is the mean difference deducible from the whole between experiment and calculation. A closer approximation between them than this, could not, I think, be anticipated even upon the hypothesis of the strict accuracy of the formula. I may also observe that if by means of the equation $f' = \frac{d}{87} \times \frac{p}{30}$, which, as we have already seen, belongs to perfectly dry air, we deduce from the precedng table 19 values of m, the mean of all will be found almost accurately equal to $\frac{1}{87}$, a result the more entitled to confidence in as much as the mean pressure for the 19 experiments being but very little over 30, and the air being perfectly dry, neither of the corrections which I investigated in my former paper requires to be applied.

If from the experiments already detailed I were to draw the conclusion that the equation $f'' = f' - \frac{d}{87} \times \frac{p}{30}$ will afford the dew-point with a degree of accuracy far surpassing ordininary hygrometrical observations, I should probably have the concurrence of most of my readers. The evidence adduced in support of the formula appears, at least to me, ample and

satisfactory. For the purpose, however, of dispelling any doubts of its accuracy which may exist in the minds of others, I undertook another series of test experiments, to the description of which I shall now proceed.

[To be continued.]

XXXIII. *A Sketch of the Geology of West Norfolk.* By C. B. Rose, *Fellow of the Royal Medical and Chirurgical Society of London.*

[Continued from p. 182.]

The Chalk Range.

THE chalk hills of West Norfolk constitute a portion of the great range of that formation extending across England in a south-westerly direction from Flamborough Head in Yorkshire to near Sidmouth in Devonshire. The greatest elevation they attain in this county is insignificant in comparison with the hills at either extremity of the range: at Sedgeford, and between Heacham and Hunstanton, the face of the country possesses some boldness of feature, and again near Holt the hill and vale are strongly contrasted: with these exceptions, the course of the chalk outcrop through the county is marked by the gently undulating surface peculiar to such districts.

I have been favoured with a communication from Captain Robe, R.E., in reply to my inquiry respecting the height of the hills in this portion of the county. After stating that the "triangulation of Norfolk was performed with instruments too small to be depended upon for the vertical angle observations," he continues, "I cannot now lay my hands on the sketch on which my judged altitudes were written; but if I recollect right, I considered the ground round Docking to be the highest, which was somewhere about 600 or 650 feet; Swaffham is probably 450 or 500, Great Massingham 600. But these are very rude guesses; indeed without using accurate instruments, it would be impossible to class the several ranges according to their relative altitudes, as they vary so very little from each other, and few of them exceeding 600 feet."

The width of the range taken at Swaffham is about seven miles; its course through the county is nearly due north and south, and its escarpment is to the west, forming generally a somewhat abrupt declivity, bounded along the greater part of its extent by a narrow valley occupied by the lowest strata of chalk, and the gault. The strata dip to the south-east, pro-

bably about five yards in the mile, and are covered by diluvium of varying thickness.

The thickness of the chalk in this county has been but recently ascertained, as appears from a communication to the Geological Society by John Taylor, Esq. It states that a well has been sunk at Diss by Mr. Thomas Lombe Taylor, and the following is the order and thickness of the beds penetrated:

1. Clay	50 ft.
2. Sand	50
3. Chalk without flints, soft and of a marly nature	100
4. Chalk with flints in layers of single stones distant about a yard from each other ...	330
5. Gray chalk, with an occasional layer of white chalk, and free from flints	60
6. Light bright blue chalk approaching to clay, with white chalk-stones	20
7. Sand	5
	615*

No. 6. must be *gault*; and 7. the *lower green-sand*.

The general surface of the chalk must have suffered prodigious abrasions from the violence of the elements, as evidenced by the immense quantity of gravel formed and collected in various situations, as well as by the different altitudes at which the chalk is found, it appearing immediately beneath the vegetable soil, even on the highest ground; and at a level not less than fifty feet lower, it may be found covered by more than 150 feet of sand and clay containing boulders.

I shall describe the chalk strata under the natural divisions, 'Chalk without Flints' and 'Chalk with Flints'.

Chalk without Flints.—Under this denomination I include all the *lower* cretaceous beds. At Hunstanton Cliff, where they are exposed, reposing immediately upon the *red chalk* (as it has hitherto been called), three natural divisions of them may be observed; the lowest is made up almost entirely of a ramose zoophite, which strongly characterizes it; the middle above has a gray colour and arenaceous texture, abounds in organic remains; the gray shale forcibly distinguishes this; and the uppermost bed, usually denominated the *lower chalk*, which here forms the upper portion of the cliff, is readily distinguished by its pure whiteness. It has been attempted to separate and arrange these beds by their *zoological* characters; the *zoophytic* bed has been considered

* Proceedings of Geol. Society, vol. ii. p. 93.

the *equivalent* of the *upper green-sand* or *firestone*; and the *gray* bed *that* of the *chalk-marl*; but I find the *characteristic* fossils of the *upper green-sand*, and *chalk-marl* so intermingled in the *gray* bed, that it is impossible to draw a line of demarcation between those two strata; and from this circumstance we are led to infer that at the epoch when the *upper green-sand and chalk-marl* of Wiltshire and Devonshire were depositing, and the then existing marine Testaceæ were entombed, similar phænomena were in progress in this portion of the great chalk basin; but the material supplied being more cretaceous, the strata consequently exhibited a dissimilar *mineralogical* character. The *lowest* of these beds (No. 4, of the section of Hunstanton Cliff) reposes upon the thin seam of dark red argillaceous matter (described page 181), which separates it from the red limestone or gault equivalent. The texture of this bed varies, some portions of it being very loose, others exceedingly hard and compact; its substance is throughout intersected by a ramose zoophyte*, the original texture of which is so completely obliterated, that it appears impossible to determine precisely the nature of what it is the relic: the formation of the stratum is best explained by supposing it originally a coral reef, and its interstices filled with cretaceous mud; it is about eighteen inches in thickness, and contains very few organic remains. Chalk resembling it in hardness and fracture occurs at Heytesbury in Wiltshire.

The next bed in the ascending series to be described, is No. 3 of the same section. Its colour is gray, and its texture less compact than the harder parts of the last-mentioned; on being fractured it exhibits an arenaceous surface, from its having a larger proportion of silex incorporated than the white chalk above. It is probable that this stratum at the cliff receives its hardness from the action of sea-water upon it; for the chalk of the same bed met with inland can be cut with a common knife, being much softer. This bed abounds with organic remains, and contains the characteristic Testacea of the upper green-sand, and also of the chalk-marl, viz. *Ostrea carinata* and *Turrilites tuberculatus.* In this bed at Hunstanton are found the *Saurian* remains; and I believe their charnel-house is at present confined to one block. The thickness of this bed varies from two feet and a half to three feet. Large flattened Ammonites occur in it, also a Nautilus; and innumerable fragments of *Inoceramus Cripsii* are scattered through it.

A stratum containing similar fossil remains is seen at Sand-

* A nearly corresponding bed, composed of ramose zoophytes, occurs in Sussex: *vide* Geology of S.-E. of England, p. 384 *note* d.

ringham, Marham, Shouldham, West Dereham Grange, Stoke Ferry, and Hockwold-cum-Wilton. The gray chalk-marl at Sandringham is of nearly as dark a shade as that of Hunstanton; and in it are found the same Ammonites, and *Pecten Beaveri* and *orbicularis*.

The same bed at Shouldham has a lighter shade, and has many ochraceous stains: at this locality blocks of an exceedingly hard nature are irregularly distributed through the softer marl; their solidity defies the blows of the quarrymen, and they are not convertible into lime by the heat of the kiln. Here I found *Ammonites Mantelli*, *Plicatula inflata*, and other shells common to the lowest beds of chalk.

At West Dereham Grange, the *Inoceramus Cripsii* (?) in fragments is very abundant, in a matrix of the same character as at the last-mentioned locality.

In the lower beds of chalk at Marham have been found two claws of *Astacus Sussexiensis*, and part of a striated tooth with fragments of bone of some Saurian animal, all of which I possess.

We are now arrived at the stratum, commonly denominated '*Chalk without Flints*'. It is a bed of much greater thickness than the last-mentioned, and forms a range of hills to the west of those of the superincumbent bed with flints, but of inferior altitude. Its course is from Hunstanton through Heacham, Sedgford, Ingoldesthorpe, Dersingham, West Newton, Hillington, Congham, Gayton, Westacre, Narborough, Marham, Barton, Stoke Ferry, Whittington to Hockwold. In all these parishes it may be examined, large pits having been opened. This chalk is sufficiently hard to be used as a building-stone, and indeed has been used for ornamental architecture. The natural divisions of the bed assume the form of oblique rhomboids, and small portions exposed to the weather separate into laminæ. Horizontal seams of argillaceous matter are occasionally interstratified with the chalk. The singular striæ observed by Mr. Mantell to occur at the natural separations of the chalk in Sussex, are also seen here, particularly in the pits at Marham and Westacre. Mr. Mantell thinks these striæ were "produced by a subsidence of the strata which caused them to slip over each other before they were entirely consolidated." May they not also be the result of concussion from remote volcanic action?

At Hunstanton Cliff, the upper portion of this bed has evidently suffered considerable disturbance. From my own examination of them, the strata rise about fifteen yards in a mile, undoubtedly the result of some disturbing force, the general rise of the strata around Swaffham being probably not

more than five yards in the mile. At Sedgeford, where hill and vale are abruptly formed, the chalk is remarkably fractured, although the strata are but little displaced.

The thickness of the "chalk without flints," exposed at Hunstanton cliff, is 28 feet: the same bed including the gray chalk is found to be 60 feet in the well at Diss.

Mineral Contents.—A few small nodules of dull white *flint* are interspersed through this bed, particularly in the upper portion. Crystallized *carbonate of lime* is at present only found lining the interior of Echini and Terebratulæ. Metalliferous substances are of rare occurrence, and consist of amorphous *iron pyrites*; *hydrate of iron* partaking of the form of a septarium, as casts of Echini, and as an ochrey powder deposited between the natural separations of the chalk; the *black oxide of manganese* is very generally scattered through the chalk of Western Norfolk in minute scales, and in dendritic forms: at the large pits in Congham, I found it also in thin seams, deposited in the vertical and horizontal partings of the chalk, and frequently mingled with the oxide of iron. In this county, the green particles (silicate of iron) usually found in the lower chalk of the southern counties, are not met with.

Organic Remains.

[Those marked with an asterisk are from the *gray* chalk.]

Name.	*Reference.*	*Locality.*
A small portion of the stem of a Lycopodite.		Marham.
POLYPI.		
Spongia paradoxica.	Geol. Norf., S. Woodw^d.	Hunstanton.
*Ventriculites radiatus. ...	Geol. Suss., t. 10—13.	Ditto.
RADIARIA.		
*Echinus saxatilis.	Org. Rem., t. 3. f. 1.	Ditto.
*Cidaris cretosa.	Ibid. t. 4. f. 3.	Ditto.
Galerites albogalerus. ...	Geol. Suss., t. 17. f. 8.	Do. Narborough, Northwold.
——— subrotundus. ...	Ibid. f. 15.	Narborough.
*——— Hawkinsii.	Geol. Norf., t. 5. f. 5.	Hunstanton, Marham.
——— ———, depressed var.	Ibid. f. 4.	Marham, Gayton.
*Spatangus hemisphæricus.	Phillips, t. 1. f. 16.	Do., Narborough, Hunstanton, Shouldham, Westacre.
*——— planus. ...	Geol. York., Phillips, t. 1. f. 15.	Hunstanton.
*Cucumerine spines.	....	Ditto, Marham.
CRUSTACEA.		
Astacus Sussexiensis	Geol. Suss., t. 30. f. 3.	Marham.
ANNELIDES.		
*Serpula antiquata.........	Min. Con., t. 598. f. 4.	Ditto, Hunstanton.
*Vermiculari Sowerbii. ...	Geol. Suss., t. 18. f. 14. 15.	Hunstanton.
CIRRIPEDA.		
α Pollicipes sulcatus ?† ...	Min. Con., t. 606. f. 7.	Shouldham.

† Posterior valve only.

CONCHIFERA.		
Exogyra difformis *MS.*	....	Marham.
Gryphæa globosa jun. ...	Geol. Norf., t. 6. f. 8.	Ditto.
Inoceramus Cuvieri.......	Min. Con., t. 441. f. 1.	Westacre.
*——— Cripsii.	Geol. Suss., t. 27. f. 11.	Hunstanton, Shouldham.
——— Brongniarti...	Min. Con., t. 441. f. 2.	Narborough, Westacre.
——— mytiloides. ...	Ibid. t. 442.	Marham, Narborough.
——— striatus.	Ibid. t. 582. f. 2.	Hunstanton.
——— intermedius....	Mag.Nat.Hist.vol.2.f.83.	Narborough.
*Ostrea carinata...........	Min. Con., t. 365.	Hunstanton.
*Pecten Beaveri............	Ibid. t. 158.	Ditto, Sandringham, Marham.
*——— asper...............	Ibid. t. 370. f. 1.	Hunstanton.
——— orbicularis.	Ibid. t. 186.	Sandringham, Ingoldesthorpe.
Plicatula pectinoides......	Ibid. t. 409. f. 1.	Shouldham.
*——— inflata...........	Ibid. f. 2.	Ditto, Marham, Hunstanton.
Terebratula plicatilis......	Ibid. t. 118. f. 1.	Marham, Gayton.
——— rostrata......	Ibid. t. 537. f. 1, 2.	Westacre.
——— undulata ...	Ibid. t. 15. f. 7.	Marham, Sandringham.
——— subrotunda.	Ibid. f. 1, 2.	Ditto, Westacre, &c.
——— semiglobosa.	Ibid. f. 9.	Ditto, ditto.
——— intermedia...	Ibid. f. 8.	Hunstanton.
*——— ovata.........	Ibid. f. 3.	Ditto, Shouldham.
*——— biplicata jun.	Ibid. t. 90.	Ditto. ditto.
MOLLUSCA.		
Cirrus depressus.	Ibid. t. 428. f. 3.	Ditto.
*Nautilus elegans†	Ibid. t. 116.	Ditto
*Ammonites peramplus....	Ibid. t. 357.	Ditto, Whittington, Narborough.
*——— Lewesiensis.	Ibid. t. 358.	Ditto, Marham.
——— Rhotomagensis.	Ibid. t. 515.	Stoke Ferry.
——— Woolgari. ...	Ibid. t. 587.	Sandringham.
——— Mantelli......	Ibid. t. 55.	Marham, Shouldham.
*——— varians.	Ibid. t. 176.	Hunstanton.
——— navicularis....	Ibid. t. 555. f. 2.	Marham.
*——— rusticus? ...	Ibid. t. 177.	Hunstanton.
*Turrilites tuberculatus....	Ibid. t. 74.	Ditto.
PISCES.		
Diodon, palatal teeth. ...	Geol.Suss., t. 32. f. 18.	Marham, Narborough, Westacre.
*Squalus *Zygæna*	Ibid. f. 4, 7, 8.	Hunstanton, Shouldham, Marham, Gayton.
——— mustelus	Ibid. f. 2, 3.	Marham, Westacre.
——— unknown..........	Geol.S.E.Eng. p 132.f.6.	Ditto.
Vertebræ small, & scales.	....	Ditto.
Iulo-eido-copros.	Geol. S.E. Eng. p. 145.	Ditto.
REPTILIA.		
*Teeth of a Saurian.........	Geol., Suss. t. 33. f. 1.	Ditto, Hunstanton.
*Bones, part of a jaw, &c.	of a Saurian ‡.	Ditto, ditto.

† It is a cast, and may be *N. simplex* of Min. Con., t. 122.
† In the possession of the Rev. E. Edwards of Lynn.

[To be continued.]

XXXIV. *On* Olbers's *Method of determining the Orbits of Comets. By Professor* Encke.

[Continued from p. 206, and concluded.]

THE method of Olbers, both in the original form in which the inventor first published it and with the modifications of the formulæ introduced by Gauss, is so generally known and spread in Germany, that at present hardly any other is used. This does not appear to be the case abroad. A proof of this is afforded by the excellent work of M. de Pontécoulant, '*Théorie Analytique du Système du Monde*,' the respectable author of which appears to have been entirely unacquainted with it. This is clearly proved not only by the entire omission of it in the chapter containing a detailed account of the calculation of the orbits of comets, but likewise by his stating "*that there are indeed some other methods, founded principally on Lambert's theorem, which, however, want the principal requisite in a practical point of view, brevity and convenience of calculation*." Nobody that has ever made a trial of Olbers's method, or has even only looked at the formulæ, will ever seriously utter such a reproach against that method.

Two or three hours are sufficient, even for unpractised computers, to determine by it the elements. In favourable cases the desired end may be obtained in one hour; and all the calculations required, together with all the auxiliary trials, do not fill a quarto page. Instead of this one, M. de Pontécoulant gives two other methods: One, peculiar to him, modified after Lagrange's, of which he says "*that it deserves to be received by astronomers, as it were, as a final result, in order to avoid loss of time and length of calculation, which other methods but too often cause**;" and the other by Laplace, which, according to the author, is in its simplest form, *without contradiction*, the most convenient which can be applied for the determination of the orbits of comets †. Now although these precise and unqualified opinions are in contradiction to one another, I hope that I shall be justified by the object of this paper and the well-merited good reception which the work of M. de Pontécoulant has met with in our country, if I succinctly explain the reasons for which I must dissent from both of them.

The method of Laplace was, before Olbers's method appeared, most frequently, nay almost exclusively, used for the

* "*Elle mérite d'être adoptée définitivement par les astronomes, qui doivent désirer d'éviter les longueurs de calcul et la perte de tems, que les autres méthodes occasionnent trop souvent.*"—tom. ii. p. 6.

† "*La méthode (de Laplace) ainsi simplifiée est sans contredit la plus commode que l'on puisse employer.*"—tom. ii. p. 493.

determination of the orbits of comets. If, therefore, in the new representation of it, the steps and the precepts of Laplace had been exactly retained, a further explanation would be superfluous. But on the very points in which M. de Pontécoulant felt himself obliged to deviate from his illustrious model, the above-mentioned opinion is founded, and these points likewise supply us with the reasons for which we must withhold our assent. It is well known that Laplace sets out from the general equations of motion, and introduces, in place of the second differentials of the coordinates, the first and second differentials with respect to time of the observed longitude and latitude, or, according to our notation, $\frac{d\alpha'}{dt}$, $\frac{d^2\alpha'}{dt^2}$, $\frac{d\delta'}{dt}$, $\frac{d^2\delta'}{dt^2}$. The method would be perfectly strict if these latter quantities could be accurately determined. But as there are no other means of finding them than the first and second differences of the observed quantities, Laplace expressly remarks, (*Méc. Cél.*, tom. i. p. 203,) that the observations must be selected and multiplied in order to obtain the data as accurately as possible; and although he gives himself a method of approximating the truth from three observations only as nearly as possible, but certainly in a circuitous manner, still he says, at the conclusion of it (*Méc. Cél.*, tom. i. p. 211), that it would be more simple and more accurate to make use of more than three observations.

In contradiction to this M. de Pontécoulant observes that it has been found by experience that the accuracy is not greater if more than three observations are used, because in this case the errors of observation have greater influence in proportion to the number employed. Practically considered this certainly cannot be admitted. If it is at all possible to derive a quantity from observations, it must be the more accurately determined the more data are employed; and should this not be the case, there is at least sufficient reason for believing that from the less number it can only be very inaccurately determined. Moreover, this remark as well as the whole discourse seem to involve the belief that errors of observation only have an influence on the more or less accurate determination of the differential quotients. But in accordance with Laplace, (*Méc. Cél.*, tom. i. p. 257,) one may easily convince one's self that even in the case of absolutely accurate observations the determination of those is always approximate, and that consequently the errors of observation contribute indeed toward the uncertainty, but are by no means the sole nor even the principal cause of it.

If for determining $\frac{d\alpha'}{dt}$ and $\frac{d^2\alpha'}{dt^2}$ only three observations are used, we have only two equations according to our notation:

$$\alpha = \alpha' - \tau'' \frac{d\alpha'}{dt} + \tfrac{1}{2}\tau''^2 \frac{d^2\alpha'}{dt^2} - \tfrac{1}{6}\tau''^3 \frac{d^3\alpha'}{dt^3} + \tfrac{1}{24}\tau''^4 \frac{d^4\alpha'}{dt^4} \ldots\ldots$$

$$\alpha'' = \alpha' + \tau \frac{d\alpha'}{dt} + \tfrac{1}{2}\tau^2 \frac{d^2\alpha'}{dt^2} + \tfrac{1}{6}\tau^3 \frac{d^3\alpha'}{dt^3} + \tfrac{1}{24}\tau^4 \frac{d^4\alpha'}{dt^4} \ldots\ldots$$

from which they are to be found. Putting

$$\alpha' - \alpha = \Delta\alpha$$
$$\alpha'' - \alpha' = \Delta\alpha',$$

we have

$$\frac{d\alpha'}{dt} = \frac{\tau^2\Delta\alpha + \tau''^2\Delta\alpha'}{\tau\tau'\tau''} - \tfrac{1}{6}\tau\tau''\frac{d^3\alpha'}{dt^3} - \tfrac{1}{24}\tau\tau''(\tau - \tau'')\frac{d^4\alpha'}{dt^4} \ldots$$

$$\frac{d^2\alpha'}{dt^2} = \frac{2(\tau''\Delta\alpha' - \tau\Delta\alpha)}{\tau\tau'\tau''} - \tfrac{1}{3}(\tau - \tau'')\frac{d^3\alpha'}{dt^3} - \tfrac{1}{12}(\tau^2 - \tau\tau'' + \tau''^2)\frac{d^4\alpha'}{dt^4}$$

In these expressions the differential quotients are independent of the greatness or smallness of the intervals of time, and are to be considered as quantities of the order 0, or as quantities of the same order with ρ, r, &c., which are to be determined by them. This will be the more the case as experience shows that their numerical value is really so considerable, that even in that respect they cannot belong to any other order.

In the ephemerides of comets when calculated for every fourth day, the fourth differences are still very sensible. But as in this case the value of 1″ in the fourth differential corresponds in general to a value

$$\frac{d^4\alpha}{dt^4} = 0{\cdot}216,$$

$\frac{d^3\alpha'}{dt^3}$ and $\frac{d^4\alpha'}{dt^4}$ will in most cases be greater than unity. Hence it follows that in the value of $\frac{d^2\alpha'}{dt^2}$, when three observations only are used, quantities of the first order are neglected whenever the intervals of time are unequal. If these latter are equal, quantities of the second order are, at any rate, neglected in the expressions for $\frac{d\alpha'}{dt}$ and $\frac{d^2\alpha'}{dt^2}$. Theoreti-

cally considered, setting aside all errors of observation, Olbers's method is more accurate by a whole order, as it neglects quantities of the second order only when the intervals of time are unequal; but when these are equal, it takes even those quantities into account. Here we have most likely the reason of the remark so frequently made in former times, that Laplace's method leads much more slowly, than the calculators wished, to a sufficiently accurate approximation: for the somewhat tedious preparatory calculation for a great number of observations and the wish to obtain as soon as possible an approximate knowledge of the orbit, generally induced calculators to use three observations only. Although the errors of observation must have a greater influence on Laplace's method, because the second differences of the observations are immediately employed, yet this greater influence alone would, in modern times, not be a motive for abandoning it, as the possible errors are now so much diminished.

M. de Pontécoulant observes, that there are cases where, to the exclusion of every other, Laplace's method must be applied *. Such cases are not known to me. Should he refer to the above-mentioned case of exception, it is clear that Olbers's method likewise answers the purpose. The course of proceeding is then analogous in both methods. By making use of a coefficient very similar to the M above used, Laplace reduces the problem to the ascertaining of two quantities, viz. r' and ρ', from two equations, one of the second, the other of the third degree. When this coefficient is too indeterminate, another unknown quantity, $\frac{d\rho'}{dt}$, is introduced, and the problem is reduced to the determination of three unknown quantities from three equations of the second, third, and fourth degree. The above-mentioned coefficient gives $\frac{d\rho'}{dt}$ as a pure function of ρ', just as M serves for determining ρ'' from ρ, and by it the quantity $\left(\frac{1}{r'^3} - \frac{1}{R'^3}\right)$ is eliminated, which, consequently, when the coefficient cannot be applied, reenters into the equation. The difference of both methods consists, therefore, in the cases of exception in Laplace's introducing the quantity $\left(\frac{1}{r'^3} - \frac{1}{R'^3}\right)$ into the final equations, as a new determining

* "*Il y a même des cas où il est indispensable de l'employer.*"—tom. ii. p. 486.

quantity, while by Olbers this quantity is not used analytically, but only according to its approximate value at the first amendment of the result. Whether in this case the method of Olbers is more convenient for calculation I do not dare to decide, as, in order to give an opinion on a matter of mere practical convenience, one ought to have had an equal experience in the use of both of them, and Olbers's method has been almost exclusively used by me. But that in all other cases, which in general are so predominant, the calculation by this latter method is, without comparison, more easy and more quick in leading to the result, appears, in the opinion of all astronomers who know both methods, to be subject to no doubt.

The course which M. de Pontécoulant takes in his own method is essentially this:

By the series above given, x, y, z, x'', y'', z'', may be expressed as functions of $x', y', z', \frac{dx'}{dt}, \frac{dy'}{dt}, \frac{dz'}{dt}, r'$, its differential quotients and the intervals of time. The same may be done with the heliocentric coordinates of the earth (let them be designated by X, Y, &c.) with regard to the coordinates of the middle place of the sun. If now from these expressions the nine equations for the geocentric coordinates are formed, three more unknown quantities, ρ, ρ', ρ'', are introduced. The author then eliminates from these nine equations the five unknown quantities, ρ, ρ'', x', y', z', and besides a quantity of the third order, which is indeed known, but very small, the quantity (0.1.2) in Gauss's *Theoria Motus*, lib. i. sect. 4, which, on account of the errors of observation, might too much affect the accuracy of the result, and then expresses $\frac{dx'}{dt}, \frac{dy'}{dt}, \frac{dz'}{dt}$, as functions of ρ'. His values have this form:

$$\frac{dx'}{dt} = \frac{dX'}{dt} + F\rho'$$

$$\frac{dy'}{dt} = \frac{dY'}{dt} + G\rho'$$

$$\frac{dz'}{dt} = H\rho'.$$

It is, however, essential to remark, that F, G, H are quite known. The quantity r' with its differentials has likewise disappeared by the other eliminations. He substitutes these values in the equation for the parabola:

$$\frac{2}{r'} = \left(\frac{d\,x'}{d\,t}\right)^2 + \left(\frac{d\,y'}{d\,t}\right)^2 + \left(\frac{d\,z'}{d\,t}\right)^2,$$

by which it assumes this form:

$$\frac{2}{r'} = \mathrm{J} + \mathrm{K}\,\rho' + \mathrm{L}\,\rho'^2,$$

in which likewise J, K, L are independent of r' and known; and combining this equation with the equation

$$r'^2 = \mathrm{R}'^2 - 2\,\rho'\,\mathrm{R}'\cos(\alpha' - \Theta') + \rho'^2 \sec\delta'^2,$$

he finds ρ' and r' by trials. Their values substituted in the expressions for $\frac{d\,x'}{d\,t}$, $\frac{d\,y'}{d\,t}$, $\frac{d\,z'}{d\,t}$, give the elements, in as much as we know now the place in space by its three heliocentric coordinates, as well as the projections of the linear velocity on the three axes. For equal intervals of time the formulæ are rather more simple than for unequal ones, but the difference in the convenience of the calculation is inconsiderable, and the process in both cases the same. The method employs quantities which are identical with, or analogous to, the numerator and denominator of M, and is therefore not applicable to the case of exception. The author calls, in this case, for other observations. In order to judge of this method it is necessary to investigate how many terms of the different series for x, y, z, x'', y'', z'', as functions of x', $\frac{d\,x'}{d\,t}$, &c., have been taken into account. This may be most clearly understood by introducing from the very beginning only so many terms as are unavoidably required, but which at the same time are the only ones that can be taken for obtaining the formulæ of the author. An examination thus conducted will prove that, supposing for the terrestrial orbit

$$\frac{d\,\mathrm{X}'}{d\,t} = -\,\mathrm{Y}'$$

$$\frac{d\,\mathrm{Y}'}{d\,t} = +\,\mathrm{X}',$$

which may be done, the formulæ of the author will be completely obtained by taking the following terms, *and no more*:

$$x = x' - \tau''\frac{d\,x'}{d\,t} \qquad x'' = x' + \tau\frac{d\,x'}{d\,t}$$

$$y = y' - \tau''\frac{d\,y'}{d\,t} \qquad y'' = y' + \tau\frac{d\,y'}{d\,t}$$

$$z = z' - \tau'' \frac{d\,z'}{d\,t} \qquad z'' = z' + \tau \frac{d\,z'}{d\,t}$$

$$X = X' \left(1 - \frac{\tau''^2}{2\,R^3}\right) - \left(\tau'' - \frac{\tau''^3}{6\,R'^3}\right) \cdot \frac{d\,X'}{d\,t}$$

$$Y = Y' \left(1 - \frac{\tau''^2}{2\,R'^3}\right) - \left(\tau'' - \frac{\tau''^3}{6\,R'^3}\right) \cdot \frac{d\,Y'}{d\,t}$$

$$X'' = X' \left(1 - \frac{\tau^2}{2\,R'^3}\right) + \left(\tau - \frac{\tau^3}{6\,R'^3}\right) \cdot \frac{d\,x'}{d\,t}$$

$$Y'' = Y \left(1 - \frac{\tau^3}{2\,R'^3}\right) + \left(\tau - \frac{\tau^3}{6\,R'^3}\right) \cdot \frac{d\,y'}{d\,t},$$

a limitation which is immediately indicated by the absence of r' in the final formulæ.

It is, therefore, clear that the author uses as many terms of the terrestrial orbit as are required in practice, and in this point his solution may be regarded as sufficient. But with regard to the orbit of the comet, he evidently supposes that the comet describes a straight line with uniform velocity. This supposition, however, which Boscovich had previously adopted *, leads in the rarest cases only to an useful approximation. It involves *two* false hypotheses, which in no case can agree with the truth, the rectilinear motion, and uniform velocity, and on that very account cannot stand by any means a comparison with the supposition which is the groundwork of Olbers's method.

The circumstance which gave rise to this paper naturally led me to these remarks. They appear to be justified and even to be called for by the warm and so well merited praise with which the work of M. de Pontécoulant has been received likewise in Germany, and which might therefore easily cause the merit of Olbers, which has remained unknown to the celebrated author, to be put into the shade by his expressions on the subject. Now, indeed, that the greatest accuracy of calculation, combined with the most convenient form, has been introduced into almost all parts of astronomy, principally by Gauss and Bessel, Olbers's method does not stand any more so preeminent among the solutions of astronomical problems. But at the time when it was first published those qualities were seldom or never found united, and it has stood the severest test of its excellence in the entire revolution of practical astronomy in this century, without experiencing any sensible effect from it.

* Olbers's *Abhandlung*, sect. 12 and sect. 16.

TABLE

FOR THE SOLUTION OF LAMBERT'S THEOREM.

η	log μ			log ν		
0,00	0,00000 00	0 18	0 36	0,00000 00	01 45	2 90
0,01	00 18	0 54	0 36	001 45	04 34	2 89
0,02	00 72	0 90	0 36	005 79	07 25	2 91
0,03	01 62	1 27	0 37	013 04	10 15	2 90
0,04	02 89	1 63	0 36	023 19	13 06	2 91
0,05	04 52	2 00	0 37	036 25	15 99	2 93
0,06	06 52	2 36	0 36	052 24	18 92	2 93
0,07	08 88	2 73	0 37	071 16	21 88	2 96
0,08	11 61	3 09	0 36	093 04	24 85	2 97
0,09	14 70	3 46	0 37	117 89	27 84	2 99
0,10	0,00018 16	3 83	0 37	0,00145 73	30 85	3 01
0,11	21 99	4 19	0 36	176 58	33 90	3 05
0,12	26 18	4 56	0 37	210 48	36 96	3 06
0,13	30 74	4 94	0 38	247 44	40 06	3 10
0,14	35 68	5 31	0 37	287 50	43 19	3 13
0,15	40 99	5 69	0 38	330 69	46 37	3 18
0,16	46 68	6 07	0 38	377 06	49 57	3 20
0,17	52 75	6 45	0 38	426 63	52 82	3 25
0,18	59 20	6 83	0 38	479 45	56 11	3 29
0,19	66 03	7 22	0 39	535 56	59 46	3 35
0,20	0,00073 25	7 61	0 39	0,00595 02	62 86	3 40
0,21	080 86	8 00	0 39	0657 88	66 31	3 45
0,22	088 86	8 39	0 39	0724 19	69 83	3 52
0,23	097 25	8 79	0 40	0794 02	73 41	3 58
0,24	106 04	9 19	0 40	0867 43	77 06	3 65
0,25	115 23	9 60	0 41	0944 49	80 78	3 72
0,26	124 83	10 01	0 41	1025 27	84 57	3 79
0,27	134 84	10 41	0 40	1109 84	88 46	3 89
0,28	145 25	10 83	0 42	1198 30	92 42	3 96
0,29	156 08	11 25	0 42	1290 72	96 49	4 07
0,30	0,00167 33	11 68	0 43	0,01387 21	100 65	4 16
0,31	179 01	12 11	0 43	1487 86	104 91	4 26
0,32	191 12		0 44	1592 77		4 37

$$\eta = \frac{[8{,}5366114]\, t}{(r+r')^{\frac{3}{2}}},\quad k = \frac{[8{,}5366114]\, t}{(r+r')^{\frac{1}{2}}}\mu,\quad \nu = \frac{[8{,}3860964]\, t \sqrt{q}}{r\, r' \sin (v'-v)}$$

η	$\log \mu$		
0,32	0,00191 12		0 44
		12 55	
0,33	203 67		0 44
		12 99	
0,34	216 66		0 45
		13 44	
0,35	230 10		0 45
		13 89	
0,36	243 99		0 46
		14 35	
0,37	258 34		0 46
		14 81	
0,38	273 15		0 47
		15 28	
0,39	288 43		0 49
		15 77	
0,40	0,00304 20		0 48
		16 25	
0,41	320 45		0 50
		16 75	
0,42	337 20		0 50
		17 25	
0,43	354 45		0 52
		17 77	
0,44	372 22		0 51
		18 28	
0,45	390 50		0 53
		18 81	
0,46	409 31		0 55
		19 36	
0,47	428 67		0 55
		19 91	
0,48	448 58		0 57
		20 48	
0,49	469 06		0 57
		21 05	
0,50	0,00490 11		0 59
		21 64	
0,51	511 75		0 59
		22 23	
0,52	533 98		0 62
		22 85	
0,53	556 83		0 62
		23 47	
0,54	580 30		0 64
		24 11	
0,55	604 41		0 67
		24 78	
0,56	629 19		0 68
		25 46	
0,57	654 65		0 69
		26 15	
0,58	680 80		0 71
		26 86	
0,59	707 66		0 74
		27 60	
0,60	0,00735 26		0 75
		28 35	
0,61	763 61		0 78
		29 13	
0,62	792 74		0 81
		29 94	
0,63	822 68		0 83
		30 77	
0,64	853 45		0 86

η	$\log \mu$		
0,64	0,00853 45		0 86
		31 63	
0,65	0885 08		0 88
		32 51	
0,66	0917 59		0 93
		33 44	
0,67	0951 03		0 95
		34 39	
0,68	0985 42		1 00
		35 39	
0,69	1020 81		1 03
		36 42	
0,70	0,01057 23		1 08
		37 50	
0,71	1094 73		1 12
		38 62	
0,72	1133 35		1 18
		39 80	
0,73	1173 15		1 24
		41 04	
0,74	1214 19		1 29
		42 33	
0,75	1256 52		1 37
		43 70	
0,76	1300 22		1 44
		45 14	
0,77	1345 36		1 52
		46 66	
0,78	1392 02		1 63
		48 29	
0,79	1440 31		1 72
		50 01	
0,80	0,01490 32		1 85
		51 86	
0,81	1542 18		1 99
		53 85	
0,82	1596 03		2 14
		55 99	
0,83	1652 02		2 32
		58 31	
0,84	1710 33		2 55
		60 86	
0,85	1771 19		2 81
		63 67	
0,86	1834 86		3 12
		66 79	
0,87	1901 65		3 51
		70 30	
0,88	1971 95		4 04
		74 34	
0,89	2046 29		4 66
		79 00	
0,90	0,02125 29		5 63
		84 63	
0,91	2209 92		7 05
		91 68	
0,92	2301 60		9 48
		101 16	
0,93	2402 76		15 27
		116 43	
0,94	2519 19		

$$\eta = \tfrac{1}{3}\sqrt{8} = 0{,}9428090$$

$$\log \mu = \log \sqrt[3]{8} = 0{,}0255763$$

$$\eta = \frac{[8{,}5366114]\, t}{(r+r')^{\frac{3}{2}}}, \quad k = \frac{[8{,}5366114]\, t}{(r+r')^{\frac{1}{2}}}\, \mu.$$

XXXV. *Proceedings of Learned Societies.*

OFFICIAL REPORT OF THE PROCEEDINGS OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, AT THE DUBLIN MEETING, AUGUST 1835.

Communicated by the Council and Secretaries.

THE contributions to science received at the annual meetings of the British Association are of two classes,—the one consisting of reports and researches executed under its immediate impulse and direction, the other of miscellaneous communications, the authors of which choose this method of bringing new facts or theories into notice, and of submitting them to public discussion.

Without undervaluing in any degree the latter class of contributions, the Association deems it advisable to deal with them in such a manner as to avoid any interference with the Transactions of other Institutions: with this view it has discouraged the production, at its meetings, of papers in a state for publication in such Transactions; and whilst it prints at full length those reports and researches which are directly its own, it has refrained from publishing the miscellaneous communications in any other form than those of notices and abstracts.

At the last meeting it was determined to draw the line of distinction still more completely, and at the same time to afford a speedier opportunity of publishing views brought forward for the sake of early notice and discussion, by transferring these abstracts from the annual Report of the Association to the periodical journals of science; and in consequence our present Number contains the commencement of a series of abstracts of the miscellaneous communications made to the Meeting in Dublin, according to the order of the Sections in which they were communicated. No attempt will be made to give an account of the verbal discussions, and yet, independent of these, the amount of the communications was such as to preclude the possibility of including the whole of the abstracts in a single number of this Journal.

As in regard to the number and value of scientific contributions, so in other respects the meeting at Dublin fulfilled all the expectations which had been entertained of its success: even before it assembled there its members had received such unusual proofs of the esteem in which the Association was held as could not but add to the spirit and animation of the meeting. The tribute to science paid by an eminent merchant of Liverpool (Sir John Tobin) in devoting one of the finest steam-boats in that port to the service of its members,

and accompanying them in their voyages as their host; the kindred spirit evinced by the Directors of the Dublin and Kingstown rail-road, who provided gratuitous conveyance from the coast to the capital; the splendid entertainments given in the Zoological and Botanic Gardens; the hospitalities of the Royal Colleges of Physicians and Surgeons, and of that illustrious academical body on which rested the chief charge and credit of receiving the Association; the participation in these festivities of the Representative of the Sovereign, and the happy manner in which he seized the occasion of conferring a public mark of distinction on the highly-gifted mathematician and astronomer who held office as one of the Secretaries of the Meeting;—in addition to these open testimonies of respect for scientific pursuits, the silent under-current of refined and invisible hospitality by which the guests of Ireland found their expenses contracted and their cheer enhanced,—all these were indeed but collateral circumstances attending that meeting, and managed in such a manner as to interfere with none of its scientific labours; but they were not ineffective in kindling a warmth of feeling by which the powers of the mind are capable of being invigorated even in the pursuit of abstract truth. The moral calm, too, which the meeting seemed to communicate,—the suspension of every feeling but that of a common interest in promoting the knowledge of nature,—this, in like manner, was but an incidental circumstance, yet it raised thoughts of the usefulness as well as the dignity of those studies which possess a charm not only to elevate the individual but to bind the species together.

Reflections of this kind, which crowd upon the mind on such occasions, and which the meeting at Dublin excited in a peculiar degree, contribute their share to that general effect of which Professor Hamilton gave so eloquent a description in his preliminary address, whilst asserting the power of social sympathy over the most private moments of exertion in the secret retirements of science. "We meet, we speak, we feel," said the Professor, "*together now*, that we may hereafter the better think and act and feel *alone*. The excitement with which the air is filled will not pass at once away; the influences that are now amongst us will not, we trust, be transient, but abiding: these influences will be with us long; let us hope that they will never leave us: they will cheer, they will animate us still, when this brilliant week is over; they will go with us to our separate abodes, will attend us on our separate journeys; and whether the mathematician's study, or the astronomer's observatory, or the chemist's laboratory, or some rich distant meadow, unexplored as yet by botanists, or some untrodden mountain top, or any of the other haunts and

homes and oracular places of science be our allotted place of labour till we meet together again, I am persuaded that those influences will operate upon us all, that we shall all remember this our present meeting, and look forward with joyful expectation to our next reassembling, and by the recollection and the hope be stimulated and supported."

Highly, however, and justly as we prize the social and sympathetic ardour of mind which these meetings spontaneously produce, we must not confine our views to this object in such a manner as to propose to dispense with more direct endeavours to effect the advancement of science. On this subject some remarks were offered by Mr. Harcourt, at the close of his statement of the Recommendations of the Committee and of the appropriation of certain sums to scientific purposes.

After adverting to some remarkable instances which had come to his knowledge of the actual effect of these meetings in awaking the dormant spirit of science, and enumerating among the indirect benefits that arise from them the means which they supply to persons whose merits have been obscured by accidental circumstances, of vindicating their own rightful claims, and of repelling that false and partial criticism by which genius had in former days been too often depressed, he proceeded to say, "After all, every important advantage which these meetings possess, and, above all, the maintenance in them of the true principles and character of philosophical investigation, will entirely depend on the continued presence and concurrence of the *master-spirits* of science; and it must be remembered that these are the persons whose attendance, from the value of their time, it is most difficult to secure. From the first commencement of the Association I have always held that there is but one motive strong enough to tear those persons from their retirements and to bind them to these annual meetings. If you here offer to them the direct and acknowledged means of advancing the science to which they are attached, if you assist the astronomer in effecting the reduction of the elements of his calculations, if you establish for the meteorologist a system of conjoint and extended observations from which the laws of the atmosphere may be deduced,—with such objects before them, the greater mastery they may possess in science the more eager will be the interest which they take in your meetings, and the more probable it is that you will enjoy the advantage of their counsel, and the communication of their spirit, than which there is nothing more essential to give life and consistence to your proceedings."

This we are persuaded is the vital principle on which the

permanence of the Association depends. Should it ever be lost sight of, should the resources of the institution come to be expended chiefly on subordinate objects, and its recommendations directed to little points, instead of the great questions which interest men of comprehensive views in the different departments of science, the consequence will be that the meetings will be left entirely to men of second-rate acquirements, and that they will speedily fall into contempt.

We have reason to hope that the next volume of the Transactions of the Association, which we are informed will soon appear, may bear evidence of a continued attention to this principle; in the mean time the answer contained in Mr. Hamilton's address to the objection of a writer in the Edinburgh Review against the exercise of the influence of the Association in obtaining from the Government a grant of money for the reduction of observations on the sun, moon, and planets, made at Greenwich by Bradley and his successors, sufficiently shows how judiciously it has commenced its operations. The astronomer royal of Ireland informs us that the particular undertaking thus objected to has afforded the most unmixed gratification to those cultivators of science who are interested in the progress of the highest department of astronomy, and he quotes the opinion of Bessel to the following effect: "To me, considering all these things together, it appears to be of the highest moment towards our future progress in the knowledge of the solar system, to reduce into catalogues, as conveniently as can be done, according to one common system of elements, the places of all the planets observed since 1750; than which labour I believe that no other now will be of greater use to astronomy."

We must refer to the Reports of the Association for further proofs, in discussions of tables of the tides and other important investigations, that there is no want of enlarged views in its Recommendations and in the expenditure of its now considerable funds. As long as this continues to be the case we have no doubt that, meet where it will, its meetings will attract a large proportion of those who are sincerely devoted to science for its own sake, and who have a just understanding of the spirit in which it is to be pursued and the methods by which it is to be advanced.

Nor does there seem to be any reason to fear that the want of a locality for such assemblages will be found to place an impediment in their way. At the late meeting there were deputies present from five of the chief commercial towns in England to invite the Association and to offer suitable accommodation in their respective towns. Bristol stood first

on the list of those from which invitations had been received on former occasions; and its situation being also far removed from the districts which the Association has hitherto traversed, it was determined to hold the ensuing meeting in that city in August next. The highly interesting and important country which forms the South-west of England will be conveniently embraced by this meeting, and the zeal which public bodies no less than individuals have shown to facilitate and encourage the arrangements for it, concurs with the high reputation of the men of science connected with Bristol, to hold out the confident expectation of a successful result.

Notices of Lectures delivered at the Evening Meetings of the Association.

Professor Powell gave a lecture on the phænomena of prismatic dispersion, in relation to the undulatory theory of light.

After giving a general view of the phænomena, and a particular description of the black lines in the spectrum whose position is taken as a measure of the refractive and dispersive powers of substances, Professor Powell proceeded to state the results of some recent labours undertaken by himself in order to ascertain whether the undulatory theory of light, which is admitted to explain almost every fact in optical science except dispersion, could be applied to explain that also*. By reducing to calculation a formula suggested to the author by Professor Airy, as arising out of the researches of M. Cauchy, and expressing a relation between the refractive index of a ray and the length of the wave, a very close agreement was found between the numbers which result from the formula and those observed by Fraunhofer for ten different media, viz. four kinds of flint glass, three of crown glass, water, oil of turpentine, and solution of potash. Professor Powell is engaged in the arduous labour of testing the applicability of M. Cauchy's modification of the undulatory theory to the explanation of the phænomena of prismatic dispersion, by individual examples; and he states, that as far as the calculations have been executed, it appears that even the extreme case of that highly dispersive substance oil of cassia is comprehended with at least considerable accuracy by the theory. It appears, then, that one of the greatest of the remaining objections to the reception of the undulatory theory is at least partially removed.

The Rev. W. Whewell stated the progress which had been made during the past year in observations of the tides, not only round the coasts of Great Britain and Ireland, but also under the direction of the Governments of Sweden, Denmark, Russia, Spain, France, Hol-

* Professor Powell's abstract of M. Cauchy's view of the undulatory theory will be found in Lond. and Edinb. Phil. Mag., vol. iv. p. 16 *et seq.*; and a notice of the results of his calculations relative to the refractive indices observed by Fraunhofer, in vol. vi. p. 374.—Edit.

land, and the United States. The dependence of the velocity of the tide wave on the depth of the ocean channels was pointed out as an instance of the collateral benefits arising from the advancement of any one branch of knowledge; for, in consequence of the perfection of this branch of hydraulical science, it might be possible for geologists to acquire some valuable information concerning parts of the ocean where no plummet ever sounded and no line was ever cast.

Mr. Babbage explained his views of a method of natural chronometry derivable from an examination of the annual layers of growth in wood. Considering these layers as liable to vary in thickness according to favourable or unfavourable seasons, and any series of them in one tree capable of being coordinated with a contemporaneous series in another, by means of these irregularities, it was shown to be possible to arrive at an accurate knowledge of the age of trees in which life had become extinct, or which had been long enveloped in peat bogs, provided a sufficient number of trees of intermediate ages, which had been subject to the same irregularities of annual growth, could be examined. The bearing of the inquiry on historical records of seasons and on geological speculations was pointed out, and the process which it would be most convenient to pursue in the application of the method clearly indicated.

Professor Sedgwick presented a general review of the labours of the geological Section during the week, illustrated by a section of the Cumbrian and Silurian systems of rocks*.

Dr. Lardner delivered a lecture on locomotive engines, illustrated by drawings and working models.

Dr. Barry gave an account of his ascent of Mont Blanc in 1834, illustrated by drawings.

Mr. Babbage described a particular phænomenon in the sea on the coast of Cephalonia (communicated to him by Lord Nugent), which appeared to indicate the existence of a large cavity below the bed of the sea, and communicating therewith.

Professor Wheatstone exhibited his speaking machine, and explained the principles of its construction, and the progress which had been made in the mechanical imitation of the human voice.

Notices and Abstracts of Miscellaneous Communications to the Sections.

MATHEMATICS AND PHYSICS.

Professor Hamilton gave a sketch of his new theory of logologues and other numbers of higher orders; (see Transactions of the Royal

* A synopsis, by Mr. Murchison, of the formations included by him in the Silurian system, was published in our number for July, present vol., p. 46.—Edit.

Irish Academy ;) also a similar account of his new theory of varying orbits.

He likewise explained to the Section the method of investigation pursued by Mr. G. B. Jerrard, for accomplishing the solution of equations of the fifth or of higher degrees*.

A short Account of some recent Investigations concerning the Laws of Reflexion and Refraction at the surface of Crystals. By Mr. M'Cullagh.

To understand the nature of the general problem which a complete theory of double refraction requires to be solved, let it be supposed that a ray of light is reflected and refracted at the separating surface of an ordinary medium and a doubly refracting crystal, the light passing out of the former medium into the latter. This limited view of the subject is taken merely for the sake of clearness of conception; since we might suppose that both media are crystallized, without increasing the difficulty of the problem. The question, it is obvious, naturally divides itself into two distinct heads. The first relates to the laws of the *propagation* of light in the *interior* of either of the two media, before or after it has passed their separating surface; and this part of the subject has been fully treated, according to their different methods, by MM. Fresnel and Cauchy. The second division of the subject had been left completely untouched. It relates to the more complex consideration of what takes place at the separating surface of the media, the laws according to which the light is there divided between the reflected and refracted rays, including a determination of the attendant circumstances indicated by the wave theory, with regard to the vibrations in the reflected and refracted rays. In the case above mentioned, when the incident light is polarized, there are four things to be determined, namely, the *magnitude* and *direction* of the reflected vibration, with the *magnitudes* of the two refracted vibrations. The four conditions necessary for this determination are furnished by two new laws, which could not be easily stated without entering too much into detail. The results, applied to determine the polarizing angle of a crystal in different azimuths of the plane of reflection, agree very closely with the admirable experiments of Sir David Brewster on Iceland spar. In the course of these experiments it was observed that the polarizing angle remained the same when the crystal was turned half round (through an angle of 180°), although the inclination of the refracted rays to the axis of the crystal was thereby greatly changed. This remarkable fact is a consequence of the theory. After some complicated substitutions in the primary equations, the value of the polarizing angle is found to contain only *even* powers of the sine or cosine of the azimuth of the plane of reflection, and therefore a change of 180° in the azimuth produces no change in the polarizing angle.

* See our last number, p. 202.—Edit.

The two new laws above mentioned, on which the theory depends, occurred to the author in the beginning of last December; but, owing to an oversight in forming one of the equations, they were not fully verified until the beginning of June.

In this theory it is supposed that the vibrations of polarized light are parallel to the plane of polarization, according to the opinion of M. Cauchy. This is contrary to the views of Fresnel, whose theory of double refraction obliged him to adopt the hypothesis that the vibrations are perpendicular to the plane of polarization. It is further supposed, that the density of the vibrating æther is the same in both media; and this hypothesis of a constant density in different media, which was found necessary for the theory, seems to accord, better than the supposition of a varying density, with the phænomena of astronomical aberration.

If we conceive the three principal indices of refraction for the crystal to become equal, we shall obtain the solution of a very simple case of the general problem with which we have been occupied,—the case of an ordinary refracting medium, such as glass. This simple case, it is well known, was solved by Fresnel. The foregoing theory leads to a simple law, expressing all the particulars of the case, but differing with regard to the *magnitude* of the refracted vibration, from the formulæ of Fresnel. The law may be stated, by saying that *the refracted vibration is the resultant of the incident and reflected vibrations*; the first vibration being the diagonal of a parallelogram of which the other two vibrations are the sides, just as in the composition of forces. The plane of this parallelogram is the plane of polarization of the refracted ray. It is to be remembered, that the vibrations in each ray are perpendicular to the ray itself, and *parallel* to its plane of polarization.

This simple case has also been considered by M. Cauchy, in a short paper inserted in the *Bulletin Universel*, tom. xiv.; but it does not seem to have been observed by any one that his solution is erroneous. His formula for light polarized parallel to the plane of reflexion, is that which belongs to light polarized perpendicular to the plane of reflexion and *vice versâ.*

Mr. Whewell read his report on the Mathematical Theories of Electricity, Magnetism, and Heat.

[This report will be printed in the next volume of the Transactions of the Association.]

On certain points connected with the recent Discoveries relative to Radiant Heat. By Professor POWELL.

In this communication the author expressed his particular satisfaction in finding that M. Melloni (in his second memoir) describes a repetition of the experiment originally made by him, and recorded in the Philosophical Transactions, 1825, with perfect success, by means of his extremely delicate apparatus. The confirmation is

the more complete, as the experiment was made by M. Melloni with a different view.

It is thus now established beyond question, that luminous hot bodies are sending out two distinct sorts of heat, or two distinct heating agents, at the same time, differing in their properties and mode of operation.

Hence the whole series of results of M. Melloni must be interpreted with reference to this distinction, and possibly the consideration of it may remove some of the apparent anomalies.

Another question of importance which has occurred to the author is this,—Whether, in the polarization apparatus, supposing one glass or pile of mica heated, it will radiate the same quantity of heat to the other in the two rectangular positions. The question is purely a mathematical one, and has been in some degree considered, at the author's suggestion, by Mr. Murphy, of Cambridge. The integration has not been completed, but Mr. Murphy thinks it clear that there will be a difference.

Abstract of a Paper on the Phænomena usually referred to the Radiation of Heat. By HENRY HUDSON, *M.D., M.R.I.A., Dublin.*

For the purpose of repeating Leslie's experiments with variations of the temperatures of the surface of the mirror and of the thermometer, the author procured a parabolic zinc mirror with a hollow back, so that its surface could be heated or cooled by filling it with hot or cold liquids.

The following are the results obtained: 1st, Whatever be the temperature of the room, if the mirror and canister be at the same temperature also, there is no effect produced by either the metallic or the varnished side of the canister. 2nd, If the canister (alone) be above the temperature of the air, the varnished side produces a greater heating effect than the metallic side, in the proportion of about 12 : 1. 3rd, If the canister (alone) be *below* the temperature of the room, the varnished side produces a greater *cooling* effect than the metallic in the *same proportion* of about 12 : 1. 4th, If the mirror be heated considerably (say to 200° Fahr.), and the thermometer so arranged that both balls are *equally* warmed by the mirror (one of them being in the focus), a canister (at the same temperature as the room) produces a *cooling* effect on the focal ball, and the varnished side displays its superior efficiency. 5th, The mirror and thermometer being as in the last experiment, the canister was heated 10 or 12 degrees beyond the temperature of the room. The effects were now found to vary according to the distance of the canister from the mirror. At a short distance it acted as a cold body, and the varnished side most efficient; on increasing the distance, the effect diminished, and at a certain point altogether ceased; the thermometer marking zero, whether the varnished or metallic side was towards it; but on increasing the distance, the

canister began to act as a warm body, and again the varnished side displayed its superiority. 6th, When the focal ball (merely) was *cooled* by the evaporation of water, or even of æther, neither side of the canister produced any change in the effect. 7th, When the focal ball was cooled 27° of Fahrenheit (by evaporation of æther), and the canister cooled 16° of Fahrenheit (being of course 11° warmer than the focal ball), the focal ball was now cooled more than previously, as if the canister were (comparatively) a cold body. The rapid evaporation of the æther makes these experiments troublesome. The author then pointed out that no theory of the *emission* of rays of heat could account for the phænomena, unless rays of cold were also admitted; and called attention to Professor Leslie's theory, as deserving further investigation, without, however, drawing any conclusion from the experiments, excepting that they could only be accounted for on *some* theory of undulations. He then suggested, as one cause of the different radiating powers of surfaces, their different capacities for heat. The two surfaces being at the *same* temperature and in the *same* medium (of a lower temperature), may be considered to have the *same* tendency to attain the common temperature of the medium, and may therefore be expected to give off the *same* portion of their excess of *temperature*, and consequently quantities of *heat* proportional to the *capacities* of the surfaces; taking the latter in the *physical* sense of having some definite thickness, which may be different in different substances.

He then mentioned a few experiments made with Melloni's thermo-multiplier, respecting the question of the "direct free transmission of heat" through rock-salt, rock-crystal, and alum. Having removed the crystals from the opening in the screen, he moved the canister (containing hot water) entirely out of the axis of the thermoscope, so that the needle stood at zero. He then placed the crystals (successively) in the opening of the screen, and found the effects on the needle to be *instantaneous*, and also to follow the same *order* in the different crystals as to its amount, as when the canister was in the axis of the thermoscope, so as to make it *questionable* whether the effects in the latter case were not (*also*) wholly owing to the *conduction* of heat through the crystals. He alluded to *these* experiments merely as indicating a method of determining the point in question: as, if there be (contrary to Melloni's deductions) *no* direct transmission of simple heat, we may expect to find the same results produced by a given source of heat, whether in or out of the axis of the instrument, provided the canister and the crystal are equally distant, and their surfaces equally inclined to each other in both cases. In the experiments with the mirror, he had used a differential thermometer containing æther instead of sulphuric acid, as being much more delicate in its indications of heat; and suggested its being made still more sensitive by the use of other liquids, having himself succeeded in making one containing *condensed* sulphurous acid gas.

On the Prismatic Decomposition of Electrical Light. *By Professor* WHEATSTONE.

The following is a brief notice of the principal results stated in this communication: 1. The spectrum of the electro-magnetic spark taken from mercury consists of seven definite rays only, separated by dark intervals from each other; these visible rays are two orange lines close together, a bright green line, two bluish green lines near each other, a very bright purple line, and, lastly, a violet line. The observations were made with a telescope furnished with a measuring apparatus; and to ensure the appearance of the spark invariably in the same place, an appropriate modification of the electro-magnet was employed. 2. The spark taken in the same manner from zinc, cadmium, tin, bismuth, and lead, in the melted state, gives similar results; but the number, position, and colours of the lines varies in each case; the appearances are so different, that, by this mode of examination, the metals may be readily distinguished from each other. A table accompanied the paper, showing the position and colour of the lines in the various metals used. The spectra of zinc and cadmium are characterized by the presence of a red line in each, which occurs in neither of the other metals. 3. When the spark of a voltaic pile is taken from the same metals still in the melted state, precisely the same appearances are presented. 4. The voltaic spark from mercury was taken successively, in the ordinary vacuum of the air-pump, in the Torricellian vacuum, in carbonic acid gas, &c., and the same results were obtained as when the experiment was performed in the air or in oxygen gas. The light, therefore, does not arise from the combustion of the metal. Professor Wheatstone also examined, by the prism, the light which accompanies the ordinary combustion of the metals in oxygen gas and by other means, and found the appearances totally dissimilar to the above. 5. Fraunhofer having found that the ordinary electric spark examined by a prism presented a spectrum crossed by numerous bright lines, Professor Wheatstone examined the phænomena in different metals, and found that these bright lines differ in number and position in every different metal employed. When the spark is taken between balls of dissimilar metals, the lines appertaining to both are simultaneously seen. 6. The peculiar phænomena observed in the voltaic spark taken between different metallic wires connected with a powerful battery were then described, and the paper concluded with a review of the various theories which have been advanced to explain the origin of electric light. Professor Wheatstone infers from his researches, that electric light results from the volatilization and ignition (not combustion) of the ponderable matter of the conductor itself; a conclusion closely resembling that arrived at by Fusinieri from his experiments on the transport of ponderable matter in electric discharges.

On the simultaneous Vibrations of a Cylindrical Tube and the Column of Air contained in it. By the Rev. James Challis.

Mr. Challis, in his report on the Analytical Theory of Hydrodynamics, and elsewhere, has expressed the opinion that, to complete the theory of musical vibrations in a cylindrical tube, it is necessary to take into account the vibrations of the tube itself. In this communication he states some results which he has arrived at theoretically, respecting the kind of influence the tube will exert on the aërial column.

It is assumed that the tube is capable of vibrating so that its particles move in planes perpendicular to the axis, with the same motion in all directions from the axis, in the same transverse section. Then, if the vibrations of the tube be of very small extent, and its diameter small, compared with its length, the following are the principal mathematical results respecting the motion of the air, so far as it is consequent upon the vibrations of the tube.

1. The motion of the particles situated on the axis will take place in the direction of the axis, and will be nearly the same as if an impulse were originally given in this direction, and the propagation were rectilinear.

2. At all points of the same transverse section, the motion, estimated in a direction parallel to the axis, will be nearly the same.

3. If the tube be made to vibrate isochronously, and so as to contain, at equal intervals along its length, nodal sections and sections of maximum vibration, it will produce in the fluid vibrations of the same duration, with points of quiescence and of maximum vibration at intervals corresponding to vibrations of that duration in *air*.

4. But unless the nodal sections of the tube be fixed, the duration of these simultaneous vibrations will not be permanent till the intervals between the nodal sections become the same in the tube as in the column of air; and then a nodal section of the tube is nearly coincident with a section of maximum vibration of the fluid.

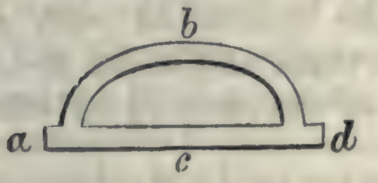
From these results it follows that there are certain transverse vibrations of the tube which will impress on the fluid column the same kind of motion as it is known can be given to it by vibrations excited near one extremity of the tube when the other is open. Mathematicians have succeeded in satisfactorily representing the circumstances of the motion in the latter case of disturbance, by assuming, from experiment, that the open end is a position of maximum vibration, or nearly so; but hitherto no distinct cause for this fact has been assigned. Mr. Challis thinks it may be shown mathematically that the aërial vibrations, excited at the extremity of the tube, and propagated along its interior, will put it into the state of vibration, which, as appears from the foregoing results, will produce an effect the same in *kind* as that observed. But to what *degree* the phænomenon may be attributed to this cause, can be learnt only from experiment, by ascertaining whether the vibrations of the tube have

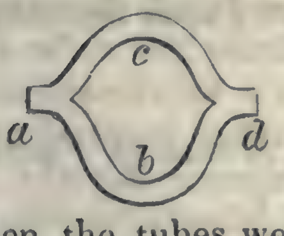
any considerable influence on the intensity of the musical sounds. The following fact seems to favour the idea of a sensible influence. A sound produced under glass, (for instance, the ticking of a French clock under a glass covering,) is *louder* than when the glass is removed, plainly by reason of the internal reflexions and the propagation of the vibrations along its surface, which cause it to vibrate so as to act with increased effect on the external air. It is not easy to discern that the glass vibrates, but the increase of sound is proved to be owing to this cause, when, on pressing the glass with the palms of the hands, the intensity is diminished. This experiment may suggest the means of detecting the influence of the vibration of a solid, in other instances of a similar nature.

Case of Interference of Sound. *By* Robert Kane, *M.D.*, *M.R.I.A.*, &c.

Among the experimental proofs of the neutralization of waves, suggested by Sir John Herschel in his interesting paper on the absorption of light*, is one which consists in transmitting through a system of canals, waves of sound, emanating from one origin, and reuniting after that by the route of one having been rendered more circuitous than that of the other, when the difference in the lengths of the paths has become such as to qualify them for interference. It occurred to Professor Kane to ascertain whether Sir J. Herschel's idea could be verified in practice, and in certain cases the result has been found satisfactory.

A system of tubes was constructed in which the lengths of the paths were as two to three. Thus in the annexed figures (which, notwithstanding the difference of shape, produced precisely the same results,) the shorter path *a. c. d.* is as 10 inches and the longer *a. b. d.* 15 inches in length. The waves of sound were generated by the Languette mechanism of an organ-pipe applied at *a.* or *d.*, and the series obtained first for each tube separately, and then from the system of both. The series of the shorter tube was found E′. E″. B″. E‴., and that of the longer tube A. A′. E″. A″. C ‴. E‴.



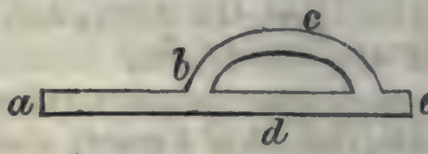


When the tubes were sounded together, the latter series was obtained complete, and the notes of the shorter tube completely suppressed. It was found, however, that the sounds of the longer tube, which also belong to the series of the shorter, were obtained with superior clearness, as E″. E‴. and A″. and B″. appeared to break into each other.

Other experiments having shown that systems of tubes may, by certain methods of vibration, be forced to produce sounds not

* Sir John Herschel's paper here referred to was published in Lond. and Edinb. Phil. Mag., vol. iii. p. 401.—Edit.

included in their natural series of harmonics, and it being possible that the suppression of the proper vibrations of the shorter tube resulted not from the ordinary principle of interference but from being forced into unison with the longer one, Professor Kane endeavoured to obtain a system in which the whole series of neither tubes should be suppressed, but that certain notes should be absorbed from the series of each. In only one case did he succeed, but in that one the result is very satisfactory. A combination was made of this figure, in which the length of the path *a. b. c. e.* was 21 inches, that of the path *a. b. d. e.* was 18 inches. The series of the shorter tube



was F. F′. C″. F‴., and of the longer D. D′. A′. D″. F″. A″. D‴. The waves being excited from the orifice *e.* the series of the system was D. F. D″. F″. A″. C‴. Hence the notes F′. and C″. had been absorbed from the series of the shorter, and the notes D′. and A′. from that of the longer tube: whilst the F. F″. and C‴. of the one and the D. D″. and A″. of the other tube maintained their place in the series given by the system.

On the various Attempts which have been made to imitate Human Speech by Mechanical Means. By Professor WHEATSTONE.

Professor Wheatstone gave an account of the various attempts which have been made to imitate the articulations of speech by mechanical means. He described and repeated the experiments of Kratzenstein, De Kempelen, the Abbé Mical, and Mr. Willis of Cambridge. De Kempelen's speaking-machine was exhibited in the course of the lecture, and made to pronounce many words and a few short sentences. Professor Wheatstone concluded with an analysis of the elements of speech founded on these and other investigations, and pointed out the importance of the inquiry as connected with philology.

On the Communication of Sound, with reference to Public Buildings. By Dr. B. REID.

Experimental Researches into the Laws of the Motion of Floating Bodies. By J. S. RUSSELL.

It was the object of these inquiries to assist in bringing to perfection the theory of Hydrodynamics, and ascertain the causes of certain *anomalous facts* in the resistance of fluids, so as to reduce them under the dominion of known laws.

The resistance of fluids to the motion of floating vessels is found in practice to differ widely from theory, being, in certain cases, double or triple of what theory gives, and in other and higher velocities, much less. These deviations have now been ascertained to follow two simple and very beautiful *laws*: 1st, A law giving a certain *emersion* of the body from the fluid as a function of the ve-

locity. 2nd, A law giving the resistance of the fluid as a function of the velocity and magnitude of a wave propagated through the fluid, according to the law of Lagrange. These two laws comprehend the anomalous facts, and lead to the following

Results.

1. That the resistance of a fluid to the motion of a floating body will rapidly increase as the velocity of the body rises towards the velocity of the wave, and will become greatest when they approach nearest to equality.

2. That when the velocity of the body is rendered greater than that due to the *wave*, the motion of the body is greatly facilitated: it remains poised on the summit of the wave in a position which may be one of stable equilibrium; and this effect is such that at a velocity of 9 miles an hour the resistance is less than at a velocity of 6 miles behind the wave.

3. The velocity of the wave is independent of the *breadth* of the fluid and varies with the square root of the *depth*.

4. It is established that there is in every navigable stream a certain velocity at which it will be more easy to *ascend* the river against the current than to *descend* with the current. Thus, if the current flow at the rate of one mile an hour in a stream 4 feet deep, it will be easier to *ascend* with a velocity of 8 miles an hour on the wave than to *descend* with the same velocity behind the wave.

5. That vessels may be propelled on the summit of waves at the rate of between 20 and 30 miles an hour.

On a Species of Balance and its Application to the Measurement of Electrical Repulsion. By W. Snow Harris.

The principle of this instrument depends on the reactive force imparted to two parallel silk threads without torsion, from which is suspended a horizontal needle or other body. If a needle be suspended by two parallel and vertical filaments of silk without torsion, equally distant from the centre, its position of rest will be horizontal, and in the vertical plane passing through the silk filaments. When the needle is turned through any given angle, the centre of gravity of the mass is raised, so that the needle will, when abandoned to the force of gravity, continue to oscillate, and will be in the state of a body falling down a small circular arc. Mr. Harris has examined the law of this force imparted to the threads, and finds it as the weight and square of the distance between the threads directly and as the length indirectly, and that it is exactly proportionate to the angle of deflection of the needle. Upon these principles Mr. Harris has constructed a balance, which he exhibited to the Section, and by which he can estimate any forces of repulsion in electricity however small. The instrument is not liable to many difficulties which embarrass the use of the torsion balance, and may be employed with advantage in many branches of experimental physics.

On Electrical Attraction. By W. Snow Harris.

The object of this paper was to examine the operation of attraction in electricity, and the laws and differences between the force of attraction actually exerted between two bodies, and the force excited in a neutral uninsulated body, by the influence of a charged body acting upon it at a distance. He endeavoured to show that the former force varied in an inverse ratio of the distance simply; that the law of the inverse square of the distance, which is the general law for the former force, does not obtain at all distances between bodies, except one of them be uninsulated and neutral; and that in all cases of attraction there are two previous forces to be considered, 1st, the force directly induced in the neutral body; 2nd, the effect of this induced force upon the charged body; which last he calls the reflected force, and attempted to prove that the whole attractive force between these bodies varies with these forces conjointly, so that if one of them becomes fixed it varies with the other. He exhibited and described several new experiments in electricity relating to electrical induction and attraction, and expressed his opinion that the whole attractive force was dependent on the action excited in the neutral bodies reflected on the charged body. This principle, with but little modification, he further applied to any case of electrical attraction whatever.

On the Application of the Proof Plane and Torsion Balance to inquiries in Electricity. By W. Snow Harris.

Mr. Harris conceives that an insulated plate of metal of small thickness may take up unequal quantities of electricity from a body and yet the distribution be uniform. The experiments in illustration of this were fully discussed. He alluded to several laws of electrical intensity attendant on the disposition of electricity on surfaces and plates varying in extension and in length, but of the same area, and endeavoured to show that contrary to the ordinary view of electrical distribution, electricity existed on both surfaces of a hollow sphere, as well as on both surfaces of a plate of the same area. He considers every case of attraction in electricity to resolve itself into the case of charging a coated non-conducting body, and that the phænomena always correspond to those observed in the latter.

On the Aurora Borealis. By Sir John Ross.

Having observed in his first arctic expedition that the aurora sometimes appeared between the two ships, and also between the ships and the icebergs, and found in his subsequent experience, both in Scotland and during the second arctic voyage, proofs satisfactory to his own mind that the aurora takes place within the cloudy regions of the earth's atmosphere, Sir John Ross states the following hypothesis on the subject, viz. " The aurora is en-

tirely occasioned by the action of the sun's rays upon the vast body of icy and snowy plains and mountains which surround the poles."

On an œconomic Application of Electro-magnetic Forces to manufacturing Purposes. By Robert Mallet.

The separation of iron from brass and copper filings, &c., in workshops, for the purpose of the refusion of them into brass, is commonly effected by tedious manual labour. Several bar or horse-shoe magnets are fixed in a wooden handle, and are thrust, in various directions, through a dish or other vessel containing the brass and iron turnings, &c., and when the magnets have become loaded with iron it is swept off from them by frequent strokes of a brush. This is an exceedingly troublesome and inefficacious process.

It appeared to the author that a temporary magnet of great power, formed by the circulation of an electric current round a bar of iron, might be substituted advantageously. The following is the arrangement which he has adopted. Several large round bars of iron are bent into the form of the capital letter U, each leg being about six inches long. They are all coated with coils of silk-covered wire, in the usual way of forming electro-magnets of such bars, and are then arranged vertically, at the interval of five or six inches from each other.

All the wires from these coils are collected into one bundle at their respective poles, and there joined into one by soldering, a large wire being placed in the midst of them and amalgamated. A galvanic battery is provided, which, if care be taken in making the junctions at the poles, &c., need not exceed four or at most six pairs of plates, of from twenty inches to two feet square. The poles of this terminate in cups of mercury, which are so placed that the large terminal wires of all the coils can be dipped into them, or withdrawn easily.

The rest of the arrangement is purely mechanical. The required motions are taken from any first mover, usually a steam engine. The previously described arrangement being complete, a chain of buckets is so contrived as to carry up and discharge over the top of the magnets a quantity of the mixed metallic particles: most of the iron adheres to the magnets, while the so far purified brass falls into a dish or tray placed beneath to receive it. This latter is also one of a chain of dishes, the horizontal motion of which is so regulated that the interval between two dishes is immediately under the magnets, in the interval of time between two successive discharges of the mixed particles on the bars.

At this juncture the communication between the galvanic battery and the magnets is interrupted by withdrawing the wires from the cups of mercury, and the result is, that the greatest part of the adhering iron drops off and falls in the space between the two dishes. The next dish now comes under the magnets, the communication is

restored, and a fresh discharge from the buckets takes place, and so the process is continued.

Some iron constantly adheres to the magnets, but this is found of no inconvenience as it bears but a small proportion to the total quantity separated.

The author has had an imperfect apparatus of the sort above described at work for some time, and has found it to answer; and suggests the application of electro-magnets for somewhat analogous objects in various manufactures. He particularly mentions needle and other dry grinding.

An Inquiry into the Possibility and Advantage of the Application of Magnetism as a Moving Power, with Remarks on the Nature of Magnetism. By the Rev. James William M'Gauley.

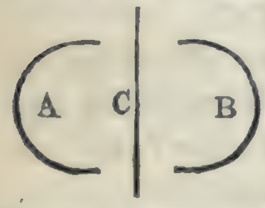
To consider with advantage the possibility of applying magnetism as a moving power, we must examine its nature and peculiar properties, because otherwise we cannot pronounce with accuracy on the quantity at our command, or the probable cheapness of its production. In this inquiry the author does not contemplate such a power as that attained in magnetic rotations and similar mechanism; it could never be advantageous; for the force of the magnet is not directly applied, or is applied at such a distance as to be almost annihilated.

The quantity of magnetism we *may produce* seems to have no limit, since we can combine any number of powerful magnets.

The œconomy of magnetism. A very small electrical power, which may be produced if necessary by the agency of sea-water, will abundantly suffice.

The obstacles likely to prevent the application of magnetism as a moving power. Of these the principal seems to consist in the disturbing influence which magnets of any power exercise over each other. This prevents the necessary reversion of the poles.

Experiment 1st. The author tried to reverse the poles of one electro-magnet in contact with another: the sudden rush of electricity evidently caused a magnetic needle near the magnet to be affected, but there was no separation or repulsion of the magnets, nor any permanent change of polarity.—Experiment 2nd. The similar poles of two electro-magnets of very different power were brought together: they attracted each other; the poles of the smaller magnet were reversed by the larger, and a counter-current was formed through its battery, and indicated by a galvanometer placed in the circuit.—Experiment 3rd. Only one of the magnets was excited, then its poles reversed; the other, acting as a keeper, was thrown off, and attracted with great violence.—Experiment 4th. Between

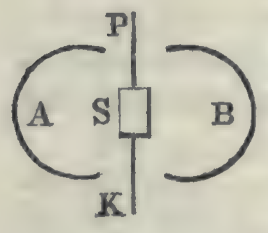


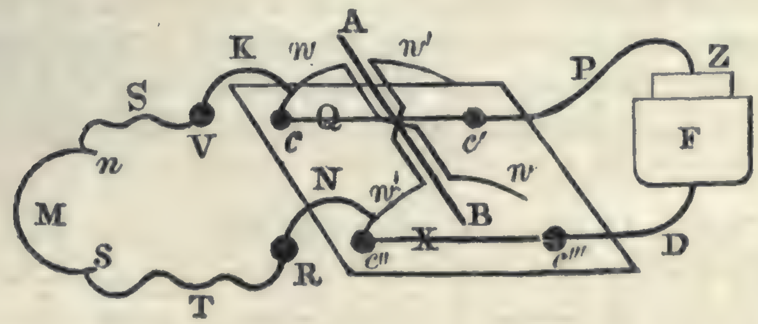
two semicircular magnets A and B, a bar of soft iron, C, was suspended, and their poles reversed in such a manner alternately as to throw off the bar from one magnet and cause it to be attracted by the other. —Experiment 5th. A bar of magnetized steel was placed between the magnets; but the effect was not so powerful,

since the iron bar became by induction a stronger magnet than the steel, and hence the mutual actions of the iron bar and the magnets was more powerful.

The very limited space within which magnetic action is confined presents a very considerable obstacle. The power is inversely as the square of the distance; at the eighth of an inch the power even of a large magnet is comparatively trifling. The stroke of one eighth of an inch, directly applied to machinery, would be nothing: we must increase the stroke, and at the same time diminish the power as little as possible. If we increase the stroke, by increasing the distance between the bar and the magnets, we diminish the power inversely as the *square* of the distance; if by applying the power of the magnet at the shorter arm of a lever, we diminish only in the inverse ratio of the *distance*: thus, if it is wanted to increase the stroke twelve times, the power in one case with the smaller distance, is to the power in the other with the larger distance, as the square of the larger is to the square of the smaller, as $12^2 : 1^2 :: 144 : 1$. With a lever, the power with the smaller distance is to the power with the larger as the distance in the latter case is to the distance in the former, $:: 12 : 1$. The power in any case is much diminished; but as we can create it in any quantity, this is of little consequence. The repulsion of the magnet for the bar, though considerable, is much less than its attraction.

The construction of the machine by which Mr. M'Gauley has exemplified the application of magnetism as a moving power is easily conceived. An oaken frame supports two magnets, A and B, horizontally. The bar P K, fixed in a strong pendulum of wood, of which S is a horizontal section, swinging on steel knife-edges, vibrates between the magnets, and has attached to its lower extremity a rod connected with the reversing apparatus and any other required machinery. The poles of the magnets are simultaneously reversed, and the bar driven with great force from one to the other, and with a velocity of two or three hundred vibrations in a minute. The apparatus for reversing the poles is simple, and can be adapted almost without increasing its weight to any combination of magnets. Let A B represent the axis upon which the wires *w w* and





w' w', crossing each other under it, are turned; these dip into cups of mercury, *c* and *c'*, connected with each other by the

wire Q, and with the zinc plate of the battery by the wire P; and into the cups c'' and c''', connected with each other by the wire X, and with the copper of the battery by the wire D. $w\ w$ and $w'\ w'$ are connected with the cups V and R by the wires K and N, and with the poles of an electro-magnet M by the wires S and T. The wires K and N rise in the cups of mercury V and R, but do not leave the mercury. Let us suppose, as in the figure, the wires $w\ w$ and $w'\ w'$ to dip into the cups $c\ c''$: we shall trace the electricity. It flows from the copper of the battery F along D to c''', along X to c'', along N to R, along T to S, where it enters one pole of the magnet M. Now, let the wires $w\ w$ and $w'\ w'$ have turned a little on their axis A B, so as to dip respectively into c' and c'''. The electricity flows from F along D to c''', along $w\ w$, crossing the axis A B, along K, to V, along S, and enters the magnet at N, before it entered at S; hence the current is inverted and the poles reversed.

The machine can be stopped or set in motion in a moment by lifting or replacing any of the wires forming the galvanic circuit. If its motion be interrupted its power is not wasted as in other machinery, but is accumulating; so that when it again works, it acts with increased power and velocity. We can continue the most perfectly uniform motion for any length of time by allowing additional fluid to drop very gradually into the copper of the battery; the one copper will answer for any number of magnets. If the zinc plates be separate, by insulating these plates with flannel bags we greatly increase the power and add to the duration of the galvanic effect.

Mr. M'Gauley has endeavoured to examine the relative lifting power of magnets of various forms. The following are some of the results:

The Iron. Three helices of the same wire, each 22 feet in length, were coiled on three different magnets, and the same battery was used.

		lb.	oz.
Magnet No.	1. In length 28 inches, diam. 2 inches, power	6	8
	2. ——— 8 ——— $\frac{5}{8}$ ———	11	11
	3. A magnet having knobs, B and C each $1\frac{3}{16}$ inches in length, diameter 1 inch, and connected by an arm $\frac{1}{16}$ by $\frac{7}{16}$, the wire coiled on the knobs, with connecting spiral; power - - -	2	4

It was found on another occasion that when the helix did not bear so great a disproportion to the iron, the power of the larger magnet was comparatively very great. To learn the best size for the iron bar suspended in the pendulum of the machine, three forms of keeper were tried.

No. 1.	$5\frac{3}{4}$ long, $\frac{15}{16}$ thick, $\frac{3}{4}$ wide; power	4	0
2.	$5\frac{1}{16}$ ——— $\frac{15}{16}$ ——— $\frac{1}{2}$ ——— ———	3	8
3.	$5\frac{7}{8}$ ——— $\frac{15}{16}$ ——— $\frac{1}{4}$ ——— ———	7	8

The large magnet would not lift a steel needle, but lifted a wire of soft iron equal in size to the needle. Perhaps the intensity of the

magnetism, though its sum was nearly equal with the same coil, was smaller when diffused through the particles of the larger magnet, and was unable to disturb the magnetic equilibrium of the steel needle.

The Battery. The magnet designated above as No. 2. was tried with a battery:

	lb.	oz.
No. 1. Double cell, each 1 foot square; power	11	11
2. ———— 1 foot by 6 inches	6	11
3. Single cell, 3 inches by $2\frac{1}{2}$	0	8
4. ———— 1 inch square	0	2

Same magnet with single cell of battery No. 2, and charged with

No. 1. 1 part sulphuric acid, 50 parts water; power....	5	0
2. 1 —— nitric acid, 50 ————....	3	8

The spark with these charges was at first very brilliant, but the effect was transitory.

No. 3. Diluted alcohol, 10 parts in 100; power........	0	4
4. 2 parts sulph. acid, 1 nitric acid, 100 water	11	10
5. 1 part ———— 2 ———— 100 ——	4	0
No. 4. was tried with a helix interposed between the positive pole of the battery and the magnet. The lifting power was	1	0
Another magnet being interposed, power	3	0
When No. 4 was exhausted, so that it would lift only 1 pound, its zinc plate was raised out of the fluid and replaced; this increased the power to	2	8
When No. 4 would lift only 1 pound its fluid was poured into its other cell, and the power became	5	9
8 pairs of plates in Cruikshank's battery caused magnet No. 4 to lift	5	8
A calorimotor of equal surface, charged with a similar fluid	11	10

Perhaps the increased tension of the electricity might be found of advantage when the helix is of great length.

In speaking of the charge, the author remarks that he is fully persuaded of the necessity of decomposition for the production of galvanic effect. A deflexion of 15° and more has been produced in a galvanometer of great delicacy, which he constructed by merely uniting, by a single corner of each, two tarnished pieces of metal, the one zinc, the other copper; the imperceptible perspiration of the hand may have acted as a fluid, and some foreign substance deposited on either or both metals have aided the decomposition.

The Helix. Four magnets with different helices were used with the same battery.

Magnet No. 1. 8 inches long, $\frac{5}{8}$ diam., coil $7\frac{1}{2}$ yards, power	11	0
2. ———————— 4 —— ——	6	0
3. That with knobs already mentioned, coiled with $7\frac{1}{2}$ yards of wire	2	5
Coiled with ribbon of copper on the knobs ..	0	2

Five magnets, each 8 inches long and $\frac{5}{8}$ diameter, were coiled with $7\frac{1}{2}$ yards of wire, and the same battery was used with each magnet.

No. 1. The wire was coiled only on the ends, and crossed straight from one pole to the other	1	0
2. Wire coiled on the ends, but connected by a spiral round the magnet	11	0
3. Wire divided, and each half placed as a helix on one end of the magnet	6	0
4. Wire coiled equally over the whole magnet	7	0
5. Wire divided into 4 equal parts, each coiled on one fourth of the magnet	5	0
6 yards of wire were coiled on a magnet $7\frac{1}{2}$ inches long, $\frac{1}{2}$ square	7	0
$1\frac{1}{2}$ yard of wire were coiled on a magnet $7\frac{1}{2}$ inches long, $\frac{1}{4}$ square	3	0

Hence the power of the magnet increases far more rapidly than proportionately with its size.

Remarks on the Nature of Magnetism. The author in this part of his paper discusses the prevalent theories of magnetism, and compares them with a variety of experiments corresponding to the analysis which he presents of the subject. It would be nearly impracticable to do justice to Mr. M'Gauley's views on the nature of magnetism in the compass of an abstract. The following brief notice will serve to convey some idea of his mode of reasoning.

Magnetism does not arise from the circulation of electrical currents, but from the electrical excitation of the mass or the particles in the magnet: not from currents, because it can begin to exist without them, can continue to exist without them, and because currents can be generated in conducting substances of sufficient quantity, velocity and intensity, without the development of magnetism. Magnetism is mere electrical excitation, provided that by mere electrical excitation we can cause its existence, and its various phænomena are such as we should expect from mere electrical excitement; and provided not electrical currents but electricity at rest be always coexistent with it. Such, the author contends, are the facts, and he proceeds to prove his position by appropriate experiments.

He then offers explanations in agreement with these views of several leading phænomena, as terrestrial induction, the mutual attraction of conjunctive wires, the position of the poles, of a permanent magnet, and of an electro-magnet, the retention of magnetism in steel, the destruction of magnetism by heat, the development of it by percussion, &c. He finally observes:

If magnetism be merely electrical excitation it is probable that, cheap as their production is at present, a more œconomical mode of forming powerful magnets may yet be discovered. Though it may be said that magnetism is not now for the first time applied to machinery, the author believes it will be acknowledged that the attempt

to apply magnetism to machinery, as an advantageous and a powerful agent, has been totally unsuccessful. In the experiment brought under the notice of the Section, the velocity with which the poles of any number of magnets are reversed is inconceivable, and the whole lifting power is applied with the greatest possible advantage directly to the mechanism; circumstances which appear to justify the author's expectation that the application of magnetism to machinery, as a moving power, will ultimately be successful.

Description of a New Dipping Needle. By R. W. Fox.

Abstract of Hansteen's Researches in Magnetism. By Capt. Sabine.

[This will be printed in the next volume of Transactions.]

Account of Magnetic Observations in Ireland. By Capt. Sabine and Prof. Lloyd; communicated by the latter.

[This will be printed in the next volume of Transactions.]

Results of three years and a half' hourly Observations with the Thermometer at Plymouth. By W. Snow Harris.

[This Paper will be printed in the next volume of Transactions.]

On the Measurement of Heights by common Thermometers. By Lieut.-Col. Sykes.

The thermometric instrument for measuring heights invented by the late Rev. F. J. H. Wollaston, though very sensible, has been found by the author and Mr. James Prinsep of Calcutta too fragile and too expensive for *rough work*. After the destruction of three of these instruments, Col. Sykes had recourse to common thermometers, which, with certain precautions, he found to answer extremely well, and having tested their indications by contemporaneous barometrical observations, he was enabled to present a table of comparative results. The thermometer to be observed was uniformly kept in the water, which was made to boil violently, about 2 inches above the bottom of the pot; two thermometers were successively employed, the difference of their scales being known; different tables of the elastic force of steam were employed in the reduction; and from the whole of the results the author has collected a few into a table, calculated to show the limits of error, of thermometric measures of heights in India, when the boiling temperature of the level of the sea is *assumed to be constantly* 212°; of single barometrical observations, when the pressure at the level of the sea is *assumed to be* 30,000 inches, (mean temperature in both cases 82°;) and of *corresponding* barometrical observations. The general agreement of all the results, by whatever method obtained, is remarkable, and is considered by Col. Sykes as justifying his opinion that common thermometers may be satisfactorily used to supply the place of barometers in measuring heights, where great accuracy is not required.

Year.	Date.	Names of Places.	ALTITUDES DEDUCED FROM 1 Jones's Bar. No. 2, with assumed pressure of 30° in., and mean temp. 82° at the level of the sea.	2 Cary's Barometer, No. 2, ditto ditto.	3 Corresponding observations with Capt. Jervis's Gilbert's Bar. and Cary's No. 2.	4 Corresponding observations with Dr. Walker's Gilbert's Bar. and Cary's No. 2.	5 Corresponding observations with Capt. Jopp and Cary's Bar. No. 2.	6 Corresponding observations with Cary's Bars. Nos. 1 and 2.	7 Corresponding observations with Cary's Bar. No. 1, and Jones's No. 2.	8 Difference of boiling temperatures, therm. 1 by	9 Difference of boiling temperatures, therm. 2 by	10 Difference of boiling temp. by Dr. Robinson's tables, Indian Navy.	11 Difference of boiling temperature, by tables of James Prinsep, Esq., Calcutta.	Difference.	12 Me.. of corresponding observations by Barometers.	13 Mean of boiling temperatures.
1827	23 May......	Highest point, Hill Fort of Poorundhur....................	4588	4599	......	......	......	†4471	4528	4536	4553	4415	4427	−16	4499	4483
1827	10 May......	Singhur Hill Fort....................	4199	4180	......	......	......	†4211	4170	4341	4220	3927	3928	−86	4190	4104
1828	15 May......	Temple at Beema Shunkur	......	......	......	......	†3090	......	......	3037	3037	2992	2991	−71	3090	3019
1825 1827	6 March... 11 May.....	Karleh, or Carlee Cave Temple..	2493	2652	......	......	......	†2530	......	2693 2526	2646 2526	2468	2478	+27	2530	2557
1827	23 May.......	Highest point of Poorundhur above Poona	2697	2681	......	......	......	†2648	†2650	2661	......	2539	2566	−64	2649	2588
1828	9 Feb.... 3 April..	Part on the Yail River..............	......	......	†2478 †2493	......	†2470	......	......	2494	2494	2480	2484	+8	2480	2486
1828 1829	....	Temple in the Hill Fort of Hureechundurghur	3972	3931	3845	†3922	†3871	†3887	3935	3840 3887	3869 3887	3824	3788	−46	3892	3846
1829	11 to 17 Decem..	Source of Kristna River at Mahabuleshwur	......	......	......	†4496 †4503	......	......	......	*4498	*4556	*4422	*4425	−24	4499	4475
1828	27 April.....	Pokree	......	......	......	......	†3197	......	......	3194	3194	3185	3141	−19	3197	3178
1828	6 April.......	Kullumb, on Goreh River	......	......	2043 2027	......	†1995	......	......	1971	2000	1988	1986	−36	2022	1986
1825 1826 1827 1828 1829	....	Poona, Hay Cottage..................	......	......	1810 1820	†1810	†1837	†1823	......	Means 1883	1897	1876	1861	+59	1820	1879
1828	16 Feb.........	Downde, on the Beema River.....	......	......	......	......	1623	......	......	1591	1591	1567	1575	−41	1623	1582
1828	29 Oct.........	Sesswur, above Poona	......	......	......	592	......	......	......	*514	*456	......	......	−107	592	485

† The heights most relied upon.

* Boiling temperatures determined by Dr. Walker.

Results of a Third Series of Experiments on the Quantities of Rain received at different Heights in the Atmosphere. By W. GRAY, Jun. and Prof. PHILLIPS.

[This Paper will be printed in the next volume of Transactions.]

Dr. Apjohn explained the substance of two papers recently read by him before the Royal Irish Academy, and which have within a few days appeared in the last part of their Transactions.* In the first of these papers a formula is investigated for pointing out what has long been considered a desideratum in meteorology, namely, the exact relation between the indications of the wet-bulb thermometer and the corresponding dew-points; while, in the second, a number of experiments are detailed, instituted for the purpose of testing its accuracy, and which seemed to prove that the formula represented observations with an extreme precision. The following is an outline of his communication, which was made orally to this Section.

When the wet thermometer attains its stationary temperature, the caloric which it loses and acquires in a given time are obviously equal. The latter is that imparted by the surrounding air to the instrument in cooling through $t - t'$ † degrees, and the former that which constitutes the caloric of elasticity of the vapour formed. Now if m be the amount of moisture which a given weight of air is capable, when saturated, of containing at the temperature t', and m' the quantity of vapour which would be formed at the same temperature by the caloric evolved from the air in cooling through $t - t' = d$ degrees, then the relation in question is expressed by the equation $f'' = f'\left(\frac{m - m'}{m}\right)$ in which f'' is the tension of vapour at the dew-point, and f' its tension at the temperature of the wet-bulb thermometer. This expression is rigorously exact, for in arriving at it we merely assume what must at once be conceded, namely, that the air which is cooled by contact with the moist bulb becomes saturated with moisture at the temperature t', and that the tension of vapour at a given volume and a given temperature is directly proportional to its quantity or specific gravity. But the value of m is easily assigned by aid of the theory of mixed gases and vapours, and that of m' also admits of being readily deduced from the known values of the specific heat of air and the caloric of elasticity of vapour. When this is done, and the proper substitutions made, the above expression is converted into the following: $f'' = f' - \frac{d}{87} \times \frac{p}{30}$.

To this solution of the dew-point problem it may be objected that the coefficient which is set down as $\frac{1}{87}$ cannot be constant, in as much as its value depends upon the latent heat of aqueous vapour

* The substance of Dr. Apjohn's first paper on this subject appeared in Lond. and Edinb. Phil. Mag., vol. vi. p. 182; and that of his second paper will be found in our present number, p. 266.—EDIT.

† t is the temperature of the air, and t' that shown by the wetted instrument.

and the specific heat of the medium which encompasses the wet thermometer, both of which vary, the former with the temperature, and the latter with the pressure and the amount of vapour present in the air. Such objection is theoretically just, and the necessary corrections have therefore been investigated by the author in his original paper, and may be applied if deemed necessary. Experience however has satisfied him that, generally speaking, they may be neglected, as in almost every instance their amount is considerably within the inevitable errors of observation.

The experiments instituted for the purpose of testing the formula, and which are detailed in the author's second paper, were next explained. The principle of the first of these is as follows: if air, in reference to which t, t' and t'' (the dew-point) have been accurately noted, be raised to any elevated temperature, and the observation be repeated in the heated air as far as respects t and t', we shall have two separate sets of observations from which to calculate the point of deposition; and as the amount of moisture in the air is not altered by the augmentation of temperature it has experienced, both calculations, provided the formula be correct, should give precisely the same result, *i. e.* the dew-point in the first instance determined by observation. Four distinct series of experiments on this plan were performed by means of a very simple apparatus, and though the depressions varied from 4°·7 to 28°·5, the calculated dew-points for each series were found almost coincident, and the differences between these and the observed dew-points were so trifling in amount as to be clearly ascribable to unavoidable inaccuracy of observation.

The next test experiments performed were suggested by the formula itself. If $f'' = f' - \frac{d}{87} \times \frac{p}{30}$, and f'' be supposed equal to 0, a condition which can only be fulfilled in perfectly dry air, $f' = \frac{d}{87} \times \frac{p}{30}$, an equation from which we deduce $d = 87 f' \times \frac{30}{p}$. Hence by determining experimentally the depression of the wet thermometer in perfectly dry air we shall be enabled to pronounce upon the validity of the general method under discussion. In order to observe several values of d, air forced from a caoutchouc bag was made to pass three times through about two inches of oil of vitriol, and then to traverse a tube containing the dry and wet thermometer, and the indications of these instruments were noted down as soon as the latter assumed its stationary temperature. Of nineteen observations of depression thus obtained, eleven were a little greater, and eight a little less than the calculated results. The mean of the plus errors of the formula was ·28, and of the minus errors ·4 of a degree, so that $\frac{\cdot 28 - \cdot 40}{19} = - \cdot 006$ is the mean difference between experiment and calculation deducible from the whole. This singularly close correspondence of theory with experiment is the more satisfactory because as the mean pressure for the nineteen experiments was but a little over 30, and as the air was perfectly dry, nei-

ther of the corrections, of which mention has been already made, required to be applied.

The most obvious method of testing the formula, or that which consists in comparing its results with the dew-points experimentally determined, was last noticed. That such criterion may be decisive, it is necessary, 1st, that the depressions be considerable in amount; 2nd, that the dew-points be accurately known. Now neither of these conditions is fulfilled by the few registers which have been published, the depressions being small, and the observations made with an instrument (Daniell's hygrometer), the difficulty of observing with which is universally admitted. It occurred, however, to the author, that both difficulties might be overcome in the following simple manner: let air saturated with moisture, and whose temperature is therefore its dew-point, be heated, and let the temperature of the heated air be taken, as also that shown by a wet thermometer subjected to the action of a current of it. Then, by the application of the formula, let the dew-point belonging to the two latter observations be calculated, and from a comparison of it with the original temperature of the air, when saturated with humidity, he expected to be enabled to pronounce with confidence upon the value of his method. Twenty-four distinct observations were thus made, the tabulated results of which justify the following conclusions: 1st, that in the case of seven of them the observed and calculated dew-points are almost coincident; 2nd, that the difference in no instance exceeds, and in but one instance reaches, one degree; 3rd, that the mean difference deducible from the whole is but ·35, or about one third of a degree of Fahrenheit.

At the close of this paper two tables are given by the aid of which the application of the formula is rendered extremely simple and expeditious.

On a New Anemometer. By the Rev. W. Whewell.

The author described the construction and purpose of an anemometer which he exhibited. The object of the instrument is to obtain a record of the total *amount* of the aërial current which passes the place of observation in each direction. The assemblage of such records for any given time will exhibit a *type* of the course of the wind for such time; the mean of such records at the same place for different years will exhibit the *annual type* of the winds for that place, and the comparison of the types of the winds for many different places will throw light upon the general annual movement of the atmosphere. Some of these instruments are now in course of construction, with a view to their being tried in different places, and it is hoped that some account of their working may be produced at the next Meeting of the Association.

Account of the Measurement of the Aberdeen Standard Scale. By Francis Baily.

[This Paper will be printed in the next volume of Transactions.]

GEOLOGICAL SOCIETY.

February 25.—A paper was first read, "On the Volcanic Strata exposed by a Section made on the site of the new Thermal Spring discovered near the town of Torre del Annunziata, in the Bay of Naples; with some remarks on the Gases evolved by this and other Springs connected with the Volcanos of Campania;" by Professor Daubeny, M.D., F.G.S., &c.

The discovery of a spring near Torre del Annunziata having occasioned the removal of a considerable portion of a cliff, a clear section has been exposed of the volcanic strata constituting that part of the base of Vesuvius. The entire height of the cliff is 68 feet, and it presents the following details:

Vegetable mould, mixed with decomposed lava, 5 to 10 feet.

Hard, compact, cellular lava, with occasionally considerable cavities, and scoriform at the bottom, 5 feet.

In one of the cavities of this stratum, Dr. Daubeny states, on the authority of Colonel Robinson, that a considerable quantity of carbonate of magnesia was found: and Dr. Daubeny also found in the same lava a white coating, which appeared to contain a very large proportion of it. The author further states that Colonel Robinson has since informed him, that in endeavouring to find the origin of the magnesia, he had excavated to the depth of 40 feet, two miles up Vesuvius, in the direction of the spring, and had found large pieces of pumice, the cavities of which were completely filled with carbonate of magnesia.

Under the bed of lava, the cliff is principally composed of strata of rapilli and scoriæ, of various shades of red, grey, and black, sometimes agglutinated by volcanic sand. In the upper portion the beds are blended together, but in the lower they are, for the greater part, tolerably distinct. In the midst of these strata is an irregular bed of compact tuff, terminating abruptly at each extremity; and at a lower level are one or two other beds of similarly constituted tuff, but traceable only for a few feet. These beds of tuff, Dr. Daubeny is of opinion, were formed on dry land, by rain or torrents, as eight or nine feet lower in the cliff, is an admixture of vegetable mould containing stems of reeds, similar to those now growing in the neighbourhood; and about one foot still lower are the roots and part of the trunk of a fir, in an upright position, in the soil in which the tree must have grown. Intermixed with the earth, found at this level, fragments of tiles, a piece of hewn timber, and other traces of human art are said to have been discovered; and at a somewhat higher level, a cypress, also in an upright position. In driving a horizontal gallery, at a level nearly 10 feet below that of the fir, vestiges of walls and buildings with fresco paintings, as well as fragments of Roman pottery, and a considerable quantity of cut marble, were discovered, proving the overwhelming, if not of another town, of at least several buildings, by an eruption of Vesuvius. The position of these buildings corresponds with that of a place mentioned under the name of Oplonti in the *Tabula Theodosiana*, and it is remarkable that the

large square building represented in that ancient map, opposite to the word Oplonti, indicates in other places a thermal spring, which the recent excavations made in this spot prove to have existed there in the time of the Romans. The catastrophe which overwhelmed the cypress and fir, above alluded to, Dr. Daubeny conceives happened prior to 572, because near Bosce-tre-case, about two miles to the north of Torre del Annunziata, and on the slope of Vesuvius, was lately discovered, a few feet below the surface, a bag of Roman gold coins, evidently almost fresh from the mint, and bearing that date; while he conceives that it may have been effected by the great eruption of 472. Considering, however, that the buildings underneath must have been overwhelmed by some eruption antecedent to that which covered the trees (these latter appearing to have grown in the very materials which enveloped the former), he is disposed to assign the formation of the beds constituting the lower part of the cliff to the eruption of 79; but he adds, it is remarkable that an event which covered the neighbourhood of Torre del Annunziata to the depth of more than 30 feet, should not have added sensibly to the accumulation of volcanic materials over Pompeii.

The spring of Torre del Annunziata possesses a temperature of about 87° of Fahrenheit, and, according to the analysis of Professor Ricci of Naples, abounds chiefly in the bicarbonates of soda, magnesia, potass, and lime, with sulphate and muriate of potass, muriate of soda, and muriate of magnesia. The quantity of carbonic acid gas discharged is so great as to maintain the water constantly in a violent ebullition, and to render the air in the stone cylinder through which it escapes unrespirable. The same gas also rises in bubbles through the sea, near the spot; and there are patches of land upon the cliff, upon which, owing to the disengagement of this gas, no plant can be made to grow. Dr. Daubeny then points out the presence of oxygen and nitrogen gas, not only in the spring of Torre del Annunziata, but in those of St. Lucia in Naples, at the Lago di Amsanti, the Agua Santa on Mount Vultur, and the Lago di Solfatara near Tivoli; the proportion of oxygen varying from 9 to 16 per cent., and of nitrogen from 84 to 91. With reference to the origin of the nitrogen gas of these and other springs, Dr. Daubeny states that he is ready to admit the possibility that it may, in many instances, be separated from the water, and not be derived from an independent source.

To those, however, who refuse to admit volcanic action to be a process of oxygenation, he says, such a mode of explaining the emission of nitrogen would seem to remove the difficulty only one step further, since it still remains to be shown why spring water, which is in general impregnated more fully with oxygen than nitrogen gas, should, in these cases, disengage chiefly the latter. He also adds, that in none of the warm springs in the neighbourhood of Naples does nitrogen appear to be evolved in any notable quantity; and that the warm springs of Ischia are destitute of all gaseous impregnation: and in conclusion, whilst admitting that the hot springs of Campania furnish no positive confirmation of the connexion between volcanic pro-

cesses and the evolution of nitrogen, the author contends, on the other hand, that they suggest no facts which can set aside the evidence in favour of that position, which the production of ammonia within the volcano itself appears to furnish.

A letter was afterwards read, from Lieut. Freyer, R.N., addressed to Charles Lyell, Esq., P.G.S., on the appearance of elevation of land on the west coast of South America.

The localities alluded to in this letter are Arica, lat. 18° 26′ south, the Island of San Lorenzo in the Bay of Callao, and Valparaiso.

Mr. Freyer states, that on his first arrival at Arica he was struck by finding shells, in very great abundance, at considerable heights above the present level of the sea. To the north of the town the coast is low, with a shingly beach and sandy plains, no rock being exposed; but he here found shells of existing species ten or twelve feet above high-water mark. On the south are interesting sections, consisting of innumerable thin beds of red sandstone and gypsum, resting upon shale, in which fragments of fossil shells were noticed. The bold promontory called the Morro of Arica is formed by the dislocation and elevation of this sandstone to the height of about 400 feet, by a mass of basalt, porphyry, and pitchstone, which pass insensibly into each other. Near the summit of the Morro the sandstone contains layers of a salt, consisting of chlorine 31·6, sodium 31·6, sulphuric acid 14·0, lime 9·45, potash and magnesia 9·0, insoluble (silex) 4·0*. South of the Morro the sandstone and gypsum beds have a small southerly dip, and form indistinct terraces towards the shore. On these terraces, wherever the rock is exposed, Balani and encrusting Millepora are found. At the height of about twenty or thirty feet above the sea they are as abundant, and almost as perfect, as on the shore; at upwards of fifty feet they still occur, but abraded by the sand constantly blowing over them; and there are traces of them at still greater heights. In the island of San Lorenzo in the Bay of Callao, Mr. Freyer found, at considerable heights, Concholepas, *Pecten purpureus*, *Sigaretus concavus*, with other shells, in great abundance, and retaining their colour almost as freshly as those living in the adjacent sea. Mr. Freyer states that he did not visit Conception, but that he had seen cargoes of the lime made from the shells found at great heights in its neighbourhood.

With respect to Valparaiso he regrets he did not more attentively examine the neighbourhood; but he says, that to the east of the town the shelly beach is now far above the reach of the tides, and that rocks were pointed out to him which he was assured were under water previously to the earthquake of 1822.

March 11.—A paper was first read, entitled "Description of a Bed of recent Marine Shells near Elie, on the Southern Coast of Fifeshire;" by William John Hamilton, Esq., Sec. G.S.

The author commences his memoir by describing the geological structure of the neighbourhood of Elie, a small fishing-town about eighteen

* The author states that this analysis was made by his friend Major Emmett, R.E.

miles north-east from Edinburgh. The promontories which form the two extremities of the bay of Elie, consist of amygdaloid and basalt, the latter exhibiting sometimes a columnar structure. Between these headlands the beach is low, and composed of alternating, thin beds of sandstone and shale, with occasionally seams of coal and strata of limestone, the whole belonging to a carboniferous system, and inclined at high angles in different directions, and without any regularity. Basalt occurs in numerous places, extending in long reefs far into the sea; the beds of sandstone and shale dipping from them on both sides: but at one point in the western part of the bay the strata are said to dip under the basalt.

About two miles to the eastward of Elie is a small promontory, near the extremity of which is situated the bed of marine shells. The extent of the deposit across the promontory does not exceed eighty yards; but its range inland could not be ascertained. The bed rests unconformably upon strata of sandstone and shale containing masses of ironstone, and consists principally of coarse sand, with rounded fragments of the sandstone and ironstone. The shells are sometimes imbedded in clay, but are more frequently scattered irregularly through the deposit, and belong, without exception, to existing species. The point at which they were first noticed, is about five feet above high-water mark, and the shells were very much broken. As the bed gradually rose towards the north-east, they were more numerous, and better preserved; the greatest height at which they were noticed, by the author, being twelve or fourteen feet above the level of high tide, and on the east side of the promontory. The deposit passes upwards into fine sand and comminuted shells. The strata, on the basset edges of which the shelly bed rests, Mr. Hamilton conceives were thrown into their highly inclined position by the agency of the neighbouring trap, and before the accumulation of the gravel and sand; but that in consequence of the angle presented by the latter, and the distribution of their component materials, a subsequent elevatory movement has taken place, to which he ascribes the difference of level between the deposit and the present shore.

A paper was afterwards read, entitled "Observations on the Diluvium of the vicinity of Finchley, Middlesex;" by Edward Spencer, Esq., F.G.S.

The district occupied by this deposit extends from Muswell Hill to Finchley Common, a distance of about three quarters of a mile: its breadth is about 150 yards, and its average thickness is from 15 to 20 feet. The best point for examining the deposit is at the gravel-pits in the lane leading from Muswell Hill to Colney Hatch. It presents, immediately beneath the vegetable soil, a bed about 14 feet thick, consisting of marl and waterworn fragments of granite, porphyry, micaceous sandstone, mountain limestone, coal, lias, oolite, and chalk, with many of the characteristic fossils of these formations. The most abundant pebbles are lias and chalk; the latter being in so great quantity as to give the whole accumulation a chalky character. Flints are likewise sufficiently numerous to be extracted for repairing the roads.

This bed is separated by a well-defined line from another of red gravel, about six feet thick, resting upon London clay. It is composed of rounded chalk flints and sand, and saurian vertebræ are occasionally found in it; but no remains of Mammalia have been noticed either in it or the superior bed. Mr. Spencer states that there appears to be, in the whole of the deposit, a total absence of the small rounded pebbles of Lickey quartz, which are plentiful on the summits of the neighbouring hills of Highgate and Hampstead: and in conclusion he suggests that the current of water which brought the materials of the upper bed into their present situation flowed from the north.

March 25.—A paper was read, entitled "Remarks on the Structure of large Mineral Masses, and especially on the Chemical Changes produced in the Aggregation of Stratified Rocks during different periods after their deposition;" by the Rev. Adam Sedgwick, F.G.S., Woodwardian Professor in the University of Cambridge.

§ 1. *Introduction.*

The first section of the paper is devoted to some general considerations of the changes produced both by igneous and aqueous agents. Changes of the former class may be effected in a comparatively short period, and can sometimes be imitated in a laboratory. But changes of the latter class have often been effected during indefinite periods of time, and under circumstances which admit not of imitation. In such cases it is by observation only, and not by direct experiment, that we can hope to rise to a rational explanation of the phænomena. The author then gives some examples of both kinds of change here considered.

§ 2. *Globular Concretionary Structure.*

The author remarks, that although this kind of structure, as seen in aqueous rocks, can seldom be imitated, yet it may be explained, in most cases, compatibly with the known modes of material action, and the phænomena may be correctly classified. He then proceeds to give examples of the structure in question.

1. *Chalk Flints.*—They are posterior to the existence of the beds in which they are found. The free siliceous matter of the formation has not been uniformly diffused, but accumulated in distinct concretions; and therefore illustrates the principle contended for in the paper.

2. *Globular Calciferous Grit, &c.*—The author dwells at considerable length on the internal structure of calciferous grits, and explains their *chatoyant* lustre by the independent crystallization of carbonate of lime through definite spaces. He then points out several cases of such rocks with a regular spheroidal structure, and with the laminations of original deposit passing, without interruption, through the several spheroids; and he infers from such phænomena that the globular structure was superinduced during the passage of the stratified mass into a solid state.

3. *Globular Magnesian Limestone.*—For a detailed account of this structure, he refers to a former memoir in the Society's Transactions,

and endeavours to confirm, by new arguments, the conclusion he drew from the phænomena, viz., that all the complicated concretions in the formation of the magnesian limestone, have been produced since the original deposition of the beds*.

4. *Rocks of Globular Structure subordinate to the Old Slate Formations of North Wales.*—He describes these rocks in considerable detail, and divides them into two classes, both of which he is disposed to arrange among stratified rocks altered by igneous action; and he remarks, that whether this opinion be true or false, the phænomena illustrate a great principle in the segregation of mineral masses.

5. *Nodular Ironstone, &c. in Beds of Shale.*—These, again, are posterior to the deposition of the beds; for it is shown (especially by some examples derived from Yorkshire) that the laminations of deposit may be traced through the nodules themselves. In this case the segregation of the nodule has often been occasioned by the presence of an extraneous body. Other examples are given of a similar chemical segregation from a similar cause; and the section concludes with an enumeration of some appearances exhibited in the mineral structure of petrifactions.

§ 3. *Slaty Structure, Cleavage, &c.*

The subjects introduced in this section of the paper are described in considerable detail. The author first compares the structure of the great Cumbrian zone, of green slate and porphyry, with the structure of the principal chain of North Wales, and shows their perfect analogy. In one respect, however, the two regions are remarkably contrasted. The Cumbrian system has few contortions or undulations, probably in consequence of the great abundance of alternating beds of porphyry; whereas a transverse section through the Welsh chain, exhibits a continued series of longitudinal anticlinal and synclinal lines. He also compares the structure of an upper slaty series in Westmoreland and Lancashire, with a corresponding upper series in North and South Wales, pointing out the circumstances in which they agree and in which they differ.

In all these regions occur many beds with a slaty cleavage, which is entirely distinguished from a jointed structure by its indefinite subdivision, and it is *never found to coincide with the true plane of stratification.* These planes of stratification and cleavage sometimes dip to the same point, and sometimes to opposite points of the compass; they are stated to be inclined to each other, sometimes at an angle less than 10°; on the average at an angle of 30° or 40°, and in no instance at 90°. Where the slaty structure is well developed, the strike of the cleavage planes coincides nearly with the strike of the beds; and this important rule holds true in countries where the beds themselves are thrown into a series of anticlinal and synclinal planes. The author adds, that there are regions in North and South Wales, thirty miles in extent, and many miles in breadth, where the cleavage planes (notwithstanding the numberless contortions of the beds) preserve an undeviating direction and dip. He states that in many large

* See Geological Transactions, Second Series, vol. iii. p. 91, *et seq.*

slate quarries there is no indication whatsoever of the true bedding; but *whenever the slates have a striped structure, the stripes* (so well known in the Cumberland and Welsh roofing-slates) *are parallel to the true beds.* To this rule there is *no exception* in the regions described; and in thousands of instances the stripes are seen to be parallel to the alternating masses of 'coarse greywacké', and to calcareous beds with organic remains.

The author then describes a flaggy, passing into a finely laminated, structure, parallel to the bedding. He points out the manner of distinguishing this from a true slaty structure, which may readily be done in a quarry, and, generally, even in examining hand specimens.

In this view, a laminated structure and a slaty structure differ so entirely in their origin, that however nearly they may resemble each other in some instances, they ought never to be confounded under the same name.

Finally, he distinguishes cleavage planes from the contorted laminæ of argillaceous schists; and endeavours to prove, by a long series of sections derived from various parts of North and South Wales, that the introduction of a crystalline cleavage was the last chemical change superinduced on the slaty deposits before they became entirely solid.

He then speculates on the enormous amount of force necessary to produce a crystalline cleavage through whole mountain chains of mechanical rocks; and supposes it due to an accumulated intensity of crystalline action in a nearly homogeneous mass, every part of which is exposed to the same conditions of aggregation while passing into its ultimate solid form. He illustrates this principle by contrasting the structure of the enormous calcareous deposits of the Alps with the structure of the thin interrupted limestone formations of England.

The foliated uneven layers of old, crystalline schists (such as chlorite schist, and mica schist) are briefly noticed, and considered generally to form portions of beds, and not of cleavage planes: but to this rule he gives some remarkable exceptions.

In all slate rocks, besides the cleavage planes, there are found one or more sets of cross-joints, which often separate the rock into regular rhombohedral solids. Even in hand specimens of such solids we may detect which is the cleavage plane, because, parallel to that plane (and to that plane only) the mass admits of indefinite subdivisions. The direction of one set of joints is generally inclined at a great angle to the direction of the beds: and, hence, as the prevailing strike of the slate rocks of England is north-east, we may expect the prevailing strike of one set of joints to be nearly north-west.

This portion of the paper concludes by recommending a more consistent use of technical language in the description of slate rocks than is commonly met with in the published works on this part of geology.

§ 4. *Jointed Structure.*

Rocks, both aqueous and igneous, have undergone a mechanical tension while passing into a solid form; and, in consequence, many of them have become subdivided by a number of parallel fissures, producing a jointed structure. Jointed pillars of basalt and prismatic

granite are considered as examples of this structure. A jointed structure of this kind may in some instances be derived from an original globular structure; but the prismatic and cuboidal blocks of granite are not considered as due to such a cause, and the concentric crusts into which such blocks are found to decompose, are regarded as the natural effects of decomposition on a mass of homogeneous structure. This conclusion is supported by the fact, that artificial pillars of granite (or even of oolitic limestone) sometimes decompose in concentric cylindrical crusts.

In the preceding cases a jointed structure is, both in its origin and in the mineral phænomena it presents, entirely distinguished from a slaty cleavage. Some granitic rocks (without a vestige of true bedding) have, however, an imperfect cleavage. The granite of St. Austell Moor is described as made up of highly inclined parallel laminations ranging about magnetic east and west; and on some parts of the region, the laminations, on approaching the schistose rocks, are extremely fine, and seem to form a passage between the killas and the granite. Such an appearance is, however, the exception, and not the rule. Again, the prismatic joints of the granite sometimes partially affect the neighbouring slate rocks. But facts like these only prove that the granite and the contiguous schists passed into their ultimate solid state at the same time, and under similar conditions, and throw no difficulties whatever in the way of the igneous theory of granite. Some writers, by confounding such joints with beds of deposit (to which they bear no analogy), have been led into most preposterous conclusions.

Many of the great parallel veins of St. Austell Moor are described as veins of segregation; yet some of them are metalliferous. Most of the metalliferous veins and cross courses of Cornwall are, however, considered by the author as veins produced by mechanical disruption; but on this hypothesis, the direction of such veins would (in part at least) be affected by the structure and direction of the rock: and from these considerations he thinks that we might (independently of any direct observations) expect in some parts of the county a system of veins running nearly magnetic east and west, and a great system of cross-joints nearly at right angles to that direction.

Before concluding the paper, he briefly notices the principal directions of the great cross courses and veins traversing the mountain limestone of Derbyshire and Flintshire. The *cross courses* are nearly in the direction of the beds, the *veins* nearly at right angles to them: and these two directions harmonize very exactly with the theory which refers both sets of fractures to a mere mechanical disruption of the rock.

April 8.—A paper was first read, entitled "Notice on the Junction of the Portland and the Purbeck Strata on the Coast of Dorsetshire;" by William Henry Fitton, M.D., F.R.S., &c.

The occurrence of silicified trunks of trees in the upright position, with their roots in a thin bed of carbonaceous clay and coarse gravel, a few feet above the top of the oolite, in the Isle of Portland, was

mentioned some years ago by Mr. Webster*. Since that time Dr. Buckland and Mr. De la Beche have inferred that both these trunks and the Cycadeæ described by the former † as occurring in the same stratum, must actually have grown in the places where their remains are found ‡; and, more recently, Professor Henslow has ascertained the existence of two other beds of clay, between that which includes the trees and Cycadeæ and the Portland stone. The author of the present notice was so fortunate as to visit the island, during the last summer, at a moment when the remains of Cycadeæ were found in one of these *lower* beds, and to see some of them before they were disturbed. The bed in which they occur is between those which are called by the quarry-men "Cap" and "Skull-cap", both of which consist of freshwater limestone; the latter being separated from the Portland stone by no more than two or three inches of clay. The Cycadeæ in this lower bed were in some cases of very large horizontal dimensions, and, like those in the dirt above the "Cap", were in the upright position, and apparently in the places where they originally grew.

The author found thin beds of clay, with more or less admixture of mechanically divided matter, alternating in several other instances with the fissile limestone at the lower part of the Purbeck formation. Even the "Cap", which in Portland is generally a continuous mass of limestone six to eight feet thick, without stratification, includes, in other places at its lower part, alternations of thin strata of clay.

The "Cap" is for the greater part compact; but it includes cavities lined with botryoidal carbonate of lime, and in other respects resembles very strongly the *travertine* of Italy. In the clay or dirt beneath it, no trees are found along with the Cycadeæ, in the Isle of Portland; but the author observed near Poxwell, on the north-east of Weymouth, part of a silicified trunk, in a bed of "dirt", which he thinks may, not improbably, be inferior to the "Cap", and, consequently, the same with the *lower* of the two beds which in Portland afford the Cycadeæ.

The author ascertained, on attentive examination, that casts of one or more species of Cypris exist throughout the whole series of the slaty limestone beds above the Portland stone; the boundary of the two formations being, as Mr. Webster had supposed, immediately below the "Skull-cap": and, generally, the portion of the Purbeck formation which adjoins that of Portland, may be said to consist of freshwater limestone, alternating with thin beds of clay and mechanically worn matter;—two of which beds, at least, contain the remains of plants standing in the places where they grew: the whole reposing upon strata which abound remarkably in marine shells.

The top of the Portland series, in which these shells are so abundant, has many points of resemblance to the recent agglomerated

* Geological Transactions, Second Series, vol. ii. p. 42.
† Ibid. p. 395. ‡ Geological Proceedings, vol. i. p. 219.

limestones of Bermuda, and of the shores of Australia, and other places in low latitudes; a fact which accords with the supposition that this portion of England was for some time in the condition of a bank, very near the surface,—or of an island of small height above the sea.

The inferences from the new facts mentioned in this paper, the author remarks, do not invalidate the conclusions of previous observers; showing only that land must have existed and produced vegetation, above the present site of Portland Island, *before* the deposition of the upper of the two beds, which contains the trees and Cycadeæ. The whole of the freshwater strata seems to have been the deposit of a lake, or an estuary, of freshwater, in which (whatever was the cause of the alternations), the waters must have deserted the strata previously accumulated at their bottom, at two successive periods at least; in each case during a space of time sufficient for the growth of Cycadeæ; and in the latter of the two cases, of trees also,—upon the surface of the land thus exposed.

In conclusion, the author suggests, that the Isle of Portland should be visited from time to time, and frequently, by geologists; since all the principal appearances of interest are presented by that part of the strata which it is necessary to remove, in order to obtain the valuable stone: so that new phænomena are continually brought to light, and as rapidly defaced, during the course of the operations at the quarries.

A paper was afterwards read, entitled "Observations on the Ichthyolites of Gamrie in Banffshire, and on the accompanying Red Conglomerates and Sandstones," by Joseph Prestwich, Jun., Esq., F.G.S.

In the summer of 1826, Mr. J. Christie of Banff, in company with Mr. Dockar of Findon, discovered the thin stratum of clay which contains the Gamrie ichthyolites, previously noticed in the bed of a small brook; but as those gentlemen did not determine the geological situation of the stratum, the author, at the suggestion of Mr. Murchison, undertook, in a recent visit to Scotland, to investigate its relative position.

The formations of which the district consists are, micaceous and argillaceous schists, old red sandstone, a red conglomerate, and lias clay and sand.

The bed containing the ichthyolites the author found to belong to the upper part of the red conglomerate, and he gives the following section of the deposit:

Soil:

1. Loose conglomerate of angular fragments of argillaceous schist, imbedded in a reddish brown, slightly micaceous sand	35 feet.
2. Red clay....................................	2
3. Grey clay, with ichthyolites disposed in nodular layers about six or eight inches apart........	4
4. Grey, slightly micaceous shale	12
5. Red conglomerate of quartz and clay slate	5
6. Coarse, micaceous, deep red sandstone........	3
7. Loose conglomerate	12

Beneath the last stratum are other beds of conglomerate, which gradually become coarser and harder, and inclose a few rolled masses of gneiss.

The ichthyolites, though most abundant in the bed of grey clay (No. 3.), are not confined to it, remains of fishes occurring in the subjacent sandstones and conglomerates; and it is only when the sandstones are entirely replaced by the conglomerates that the fish exuviæ disappear. This distinction the author assigns to the apparent turbulent state of the water which brought the conglomerates together, and the comparatively tranquil condition of that which deposited the sandy strata.

With respect to the geological position of this system of sandstones and conglomerates, the author shows that it rests unconformably on the old red sandstone, and that it is overlaid by outliers of lias; he is further of opinion that it belongs to the age of the coal measures, and, probably, of the millstone grit. This conclusion is likewise in accordance with the opinion of M. Agassiz respecting the age of the deposit, deduced from the characters of the ichthyolites.

The author, in addition to this account of the bed containing the fish, gives a detailed description of the old red sandstone, the schistose formation, and the trap rocks of the district. He gives a detailed account, also, of the faults, and shows by sections, that those between Findon and Gamrie, and near Gamrie church, are older than the lias, because they are overlaid by that formation, without affecting it.

April 29.—A paper was first read, entitled "Remarks to illustrate Geological Specimens from the West Coast of Africa," by Captain Belcher, R.N., F.G.S., &c., an abstract of which is given in No. 40 of the Society's 'Proceedings.'

A paper was next read, entitled "A Description of Specimens collected on the Island of Ascension by the Rev. W. P. Hennah;" communicated by the Rev. Richard Hennah, F.G.S.; of which also an abstract is given in the 'Proceedings.'

A paper was afterwards read, "On a Bed of Gravel containing Marine Shells of recent species, at 'The Willington', in Cheshire;" by Sir Philip Grey Egerton, Bart., M.P., V.P.G.S.

"The Willington", the residence of Major Tomkinson, is situated at the western base of the "Forest Hills", four miles north of Tarporley, and about nine miles in a direct line from the nearest point on the shore of the Mersey. The "Forest Hills" belong to the new red sandstone formation, and at this point have an elevation of 120 or 130 feet above the Mersey, or from 50 to 60 feet above the adjacent valley. In the summer of 1834 a bed of gravel was exposed close to their base, to the extent of three or four yards, and to the depth of one; and the author ascertained that it was between seventy and eighty feet above the level of the Mersey. It presented so different a character from the usual gravel of Cheshire, that it attracted the attention of the workmen. It was composed principally of fine-grained gravel and pebbles from one to six inches in diameter, intermixed with sand and fragments, and sometimes perfect shells of ex-

isting marine species. The pebbles had undergone considerable attrition, but presented a flattened, not a spheroidal form. They consisted chiefly of granite, slate, chert, porphyries, greenstone, amygdaloid, new red sandstone, coal-measure sandstone, and quartz pebbles, identical with those of the conglomerate beds of the new red sandstone of the district. The shells, which were thickly disseminated through the whole deposit, were in an extremely friable condition, and belonged to *Turritella terebra, Cardium edule,* and *Murex erinaceus,* as well as to a thin smooth bivalve, the genus of which could not be determined. The extent or thickness of the deposit the author was not able to ascertain, though he was informed that in making a well at "The Willington", twelve yards of gravel and sand were penetrated before the new red sandstone was reached; he could not, however, learn if any Testacea had been noticed in making the well.

The shelly bed was separated by a well-defined line from an overlying deposit, twenty feet thick, of the ordinary diluvium of Cheshire, and consisted principally of sand, containing pebbles and boulders of granite, slate, greenstone, and other rocks.

From "The Willington" the country slopes very gradually to the Gowey, a small sluggish stream which conveys the drainage of this part of Cheshire to the river Mersey, and empties itself into that river near the village of Ince. The author then quotes some extracts from the Red Book of St. Warburgh's Abbey, given in "Ormerod's Topographical Account of Cheshire," stating that in Wyrall, in the manor of Ynes, the sea had removed thirty caracates of land, and was daily destroying more: the author also states that, according to popular tradition, the sea once occupied a large portion of the valley at the foot of the Forest Hills.

From the above details, and from a careful examination of all the facts he could collect, he gives the following, as the conclusions at which he has arrived:

1st. That the bed of gravel was deposited on the shore of the ancient sea, at that period extending to the base of the Forest Hills.

2nd. That this has occurred since the existence of some of the species of shells now inhabiting our seas.

3rd. That an alteration in the relative levels of land and sea, to the amount of seventy feet, has taken place since its deposition.

4th. That it has been covered by an accumulation of diluvium twenty feet in thickness.

A communication was lastly read, entitled "Notice of a newly discovered gigantic Reptile;" by the Rev. William Buckland, D.D., F.G.S., &c.

The remains noticed in this communication were discovered near Buckingham in a bed of clay immediately above the cornbrash; and the author states that their preservation is owing to the zeal of William Stowe, Esq., of that town. The principal bone is a caudal vertebra of a reptile larger than the Iguanodon. It measures about six inches in its longitudinal diameter, and six inches in the vertical and largest transverse diameters of its articulating faces. Both these faces are slightly convex, and are smallest on the lower side, and

depressed on the upper, to form the channel for the spinal marrow. The body of the vertebra is much compressed towards its centre, and the transverse processes are reduced to a small tubercle on each side. On the inferior margin of the articulating surfaces are large oblique facets for the reception of a powerful chevron bone. The form of this vertebra differs essentially from the subquadrangular form of the caudal vertebræ of the Iguanodon, and it has no perforations on the inferior part of its body, like those which enter the lower side of the body of the vertebræ of the Plesiosaurus.

Other bones, of corresponding size, and considered by Dr. Buckland as belonging probably to this genus, have been found at Bradwell, a few miles north-east of Buckingham, on the continuation of the same formation.

ZOOLOGICAL SOCIETY.

April 28.—The Chairman exhibited a portion of the vertebral column of a *Sole, Solea vulgaris*, Cuv., which had been sent to him by Sir Thomas Phillipps, Bart., for the purpose of illustrating the manner in which reunion takes place after fracture of the long spinous processes of the caudal *vertebræ*. Each end of the fractured bones is enlarged, and appears to have become a new centre of ossification, from whence processes have been sent out to join the neighbouring one; and where, as in this instance, several adjoining bones have partaken of the injury, the new processes have, in more than one place, united the broken portion, not to that with which it was originally connected, but to the bone immediately preceding or following it: the new bone exhibiting no appearance of disease, but possessing altogether a healthy character.

Mr. Gray exhibited a specimen of a *Toad,* which he had recently received from Swan River, whence it was sent to him by Joseph Wright, Esq. Believing it to be hitherto undescribed, he characterized it as the BOMBINATOR *Australis.*

The back is generally smooth, and has some small smooth tubercles arranged along it in longitudinal series. The toes are four in number on the anterior feet, and five on the posterior: they are slender, free, and unequal.

Mr. Gray remarked, that the form of *Toad* to which the name of *Bombinator* has been given had not previously been met with beyond the limits of Europe; and added, that this Australian species agreed with the European, not only in the essential characters of the group, but in the tone and nature of its colouring, and was only specifically distinguishable by the mode in which the markings were distributed on its surface.

Mr. Gray also exhibited some specimens of the genus *Echinus,* as restricted by Lamarck and modern authors; and proceeded to explain his views with regard to its subdivision into what he considers four natural genera, adapted to facilitate the distinction of the species of this extensive group. He regards this distinction as of the more importance, in as much as some of the characters which had been

used for this purpose, such as the number of the *tesseræ,* and of the pores in the *ambulacra,* have been found to be inconstant; the number of these increasing, as they are now known to do, with the age of the specimens. He proposed to divide the *Echini* as follows:

Genus 1. Arbacia.

Corpus depressum.

Areæ ambulacrorum angustissimæ: *ambulacra* angusta, recta, singulo e serie simplici tesserarum biporosarum superpositarum efformato.

Tesseræ ovariales et *interovariales* mediocres.

Anus valvis quatuor spiniferis tectus.

This genus corresponds with *Echinus* section A. of M. de Blainville, and contains *Arbacia pustulosa* (*Echinus pustulosus,* Lam.), *Arb. punctulata* (*Ech. punctulatus,* Lam.), &c.

Genus 2. Salenia.

Corpus subsphæricum.

Areæ ambulacrorum angustissimæ: *ambulacra* angusta, recta, singulo e serie simplici tesserarum biporosarum superpositarum efformato.

Tesseræ ovariales et *interovariales* maximæ.

Anus subexcentricus.

This genus is known only in the fossil state, and has hitherto even been confounded with *Cidaris,* but its tubercles are not pierced. It comprehends *Salenia scutiger* (*Cidaris scutiger,* Munst., Goldf. Petref., t. 49. f. 4.—Park., Org. Rem., t. 12. f. 13.; *Echinus petaliferus,* Desm.) and two or three other allied species in Mr. Gray's collection.

Genus 3. Echinus.

Corpus plus minusve depressum.

Areæ ambulacrorum latitudine dimidium arearum extraambulacralium æquantes: *tesseræ ambulacrales* tripliciter biporosæ.

Tesseræ ovariales et *interovariales* mediocres.

Anus subcentralis, squamosus; squamis spiniferis.

The ambulacral *tesseræ* in this genus may be regarded as being each composed of three doubly pierced pieces: of these the upper is placed in the middle of the upper edge of the *tessera*; the next below it, on the middle of the outer edge; and the lowest on the lower part of the inner edge of the plate: so that when the plates are together, forming the *ambulacra,* the pores appear to form oblique lines, each composed of three double pores, the inner upper double pore of each line belonging to the plate above the other two double pores.

This genus contains the sections B*. C. E. and G. of M. de Blainville. The species may be divided into two very distinct sections, thus:

1. *Ambulacris angustioribus: poris mediocribus approximatis.*
   a. *Ore subintegro.*

Of this section *Ech. esculentus* may be regarded as the type.

On this species Mr. Gray incidentally remarked that it is extremely variable in shape, becoming very high and subconical in the adult age, when it is *Ech. Melo,* Lam.; and being often subangular, in which condition it is *Ech. subangulosus,* Ejusd.

b. *Ore profundè inciso.*

Ech. excavatus, Lam.; *Ech. Pileolus,* Lam.; &c.

2. *Ambulacris latis: poris inter se tuberculis parvis sejunctis: ore 5-inciso.*

Ech. ventricosus, Lam.; &c.

Genus 4. Echinometra.

Corpus plus minusve depressum, sæpè oblongum.

Areæ ambulacrorum mediocres: *tesseræ ambulacrales* quinquariam vel ultra biporosæ.

Tesseræ ovariales et *interovariales* mediocres.

Anus subcentralis, squamosus; squamis sæpe spiniferis.

In this genus the ambulacral plates may be considered as being composed of five or more doubly pierced pieces, which form an arched line round the outer edge of the *tessera,* with a single pair of pores at its lower inner angle.

The spines with which the species of this genus are furnished are often of very unequal size, and they are of very variable form, some of the larger ones being very long, as in *Echinometra trigonaria*; and others very short and truncated, as in *Ech. atrata.*

Mr. Gray stated that he had formerly separated from the *Echini* some of the species of this genus which are peculiar for their oblong form, and that the genus so proposed by him had been adopted by M. de Blainville; but a much more extended examination has convinced him that individuals of the same species vary from roundish to oblong: and, therefore, having observed many round species agreeing with oblong ones in the peculiar character of the *ambulacra,* he has united them to the former, under the same name. It is to be remarked, as throwing doubt on the bilaterality of the *Echinidæ,* attempted to be established by M. Agassiz*, that the spongy ovarial plate which that gentleman regarded as the mark of the hinder part of the *Echinidæ,* is always placed on one side or the other of the longer axis of the oblong species.

This genus will contain sections B**. D. and F. of M. de Blainville, as well as the *Echinometra* of that author, and many new species which are as yet undescribed.

Mr. Gray subsequently exhibited a specimen of a new genus of *Corals,* which he had recently received from the coast of Montserrat in the West Indies. The coral in question is formed almost entirely of rather large transparent rough fusiform *spicula,* which are irregularly placed side by side along the stems, and are imbedded in the animal matter: the *spicula* are so abundant as to render the coral

* The Memoir of M. Agassiz on this subject will be found in Lond. and Edinb. Phil. Mag., vol. v. p. 369.—Edit.

very hard, and to give it much of the appearance of a mass of arragonite, of which it has also the form. Its stem is irregularly cylindrical, rather crooked, and slightly tapering: it throws off a rather thinner branch a little below the middle of the main stem; and both the main stem and its branch end in a hemispherical head, the upper surface of which is covered with forty or fifty rather large conical tubercles, each terminating in a small central mouth. These tubercles are formed of *spicula* resembling those of the stem, the points of which arm the *apices* of the cones. The central cones are the largest and most distinct, and the marginal ones are smaller, and more or less confluent. The stem when broken exhibits similar *spicula* and a few internal cells, but it has no distinct central *axis*: the conical tubercles of the head are hollow, and they doubtless inclose and give exit through their central mouths to the *Polypes* which form the coral.

This coral appears to be most nearly allied to the genus *Zenia* (of which *Alcyonium floridum* of Esper is the type), and agrees with it in having no distinct *axis*, and in having the whole surface covered with large *spicula*, and the *Polypes* protruded from tubular cells at the end of the branches. It differs, however, from that genus in its *spicula* being much more abundant, and the coral consequently more solid, and by no means spongy; and in being less branched, with the polype-cells forming a hemispherical head, instead of a bunch of small branches. For these reasons Mr. Gray is led to consider it as forming a new genus, which, until the animal is known, he is induced to place next to *Zenia*, with the following characters:

Genus NIDALIA.

Corallium fixum, cylindricum, subramosum, subsolidum, spiculis calcareis densè indutum; apice capitato, hemisphærico, e papillis conicis inæqualibus spiculiferis formato.

NIDALIA OCCIDENTALIS. *Nid. corallio albido, subramoso.*

Hab. in littore Oceani Atlantici apud Montserrat in Indiâ Occidentali.

The specimen described is now in the collection of the British Museum.

XXXVI. *Intelligence and Miscellaneous Articles.*

ON THE EXISTENCE OF ARSENIC IN PHOSPHORUS.

M. HERTN, a druggist of Berlin, found that some phosphoric acid, prepared according to the Berlin Pharmacopœia, by treating phosphorus with nitric acid, became of a yellow colour after some time, on the addition of sulphuretted hydrogen. M. Barwald passed a current of sulphuretted hydrogen through phosphoric acid, prepared by the method above mentioned: from a pound of acid he obtained eight grains of a precipitate, which being mixed with carbonate of soda was decomposed in a glass tube by dry hydrogen. In the upper part of the tube a metallic layer was deposited, which from its appearance, and also from the odour of garlic which it emitted when thrown on sea

coals, was unquestionably determined to be metallic arsenic. Some other portions of phosphoric acid, procured from other druggists at Berlin, gave the same results. M. Barwald satisfied himself that neither the vessel used, the nitric acid nor the sulphuretted hydrogen contained any arsenic, and that this metal came from the phosphorus. He learnt from another druggist that water in which phosphorus was long kept also contained arsenic. This fact was confirmed by Wittstock; but phosphorus which he himself prepared did not contain any arsenic. His experiments also showed that a considerable quantity of arsenic might be mixed with phosphorus without materially altering its appearance; but, according to the proportion of the arsenic, its colour was deeper and of a more marked yellow gray, especially at the surface: if the quantity of arsenic was very considerable, its colour was steel gray, but it was soft and ductile like wax. M. Barwald attributed the presence of this metal in the phosphorus to the sulphuric acid employed to prepare it. M. Liebig also satisfied himself that the phosphorus bought of the druggists of Frankfort contained rather a large quantity of arsenic. He found, as the chemists already mentioned had done, that during the oxidation of the phosphorus by the diluted nitric acid, phosphorous acid is principally formed; and he also observed that when this acid solution was evaporated to expel the nitric acid, there was developed phosphuretted hydrogen when it had arrived at a certain degree of concentration, and this reduced the arsenic or arsenious acid which it contained, and a black heavy powder was deposited, which was metallic arsenic. M. Liebig proposes, in order to purify phosphoric acid, to substitute phosphorous acid for sulphuretted hydrogen, this latter requiring several days for its action. The process which he proposes is the following: oxidize two parts of phosphorus by dilute nitric acid, and evaporate the liquor until the arsenic is deposited; at the same time there is to be placed in a funnel, deposited in a cellar, one part of phosphorus in a glass tube; the phosphatic acid which is obtained is to be used to purify the phosphoric acid a second time when diluted with water: the mixed liquors are to be evaporated, and if arsenic is again deposited, the operation is to be repeated until no effect is produced by the addition of phosphatic acid.

The above is extracted from the *Annalen der Pharm.* 1834. The editor of the *Journal de Chimie Médicale* adds, that some experiments which he had made showed that some phosphoric acid which was prepared fifteen years since contained arsenic, but they had not found it in any other specimen, nor in the water in which phosphorus had been kept for four years.—*Journal de Chimie Médicale*, April 1835.

COMPOSITION OF OXYCHLORIDE OF ANTIMONY. BY J. F. W. JOHNSTON, F.R.S.E., &C., PROF. CHEM. IN THE UNIV. DURHAM.

Professor Johnston remarks that "we have many analyses of this substance, no two of which agree, obviously because having washed it more or less, each chemist operated upon a mixture in which the relative quantities of oxide and chloride of antimony actually differed.

According to Bucholz it contains $4\frac{2}{3}$ per cent. of dry muriatic acid; according to Grouvelle it consists of 7 atoms oxide united to one atom of chloride of antimony *. Dumas † gives as its composition 82 of chloride and 18 of oxide of antimony; while Mr. Phillips found it to contain 7·8 per cent. of muriatic acid, and 92·2 [92·45] of oxide of antimony, or to consist of one atom of acid to $5\frac{1}{2}$ of the base ‡. From results so discordant, it is impossible to draw any conclusion in regard to the true nature of the compound, or whether there be any constant and definite compound of the chloride with the oxide of antimony. That oxides and chlorides do unite there is now no doubt, but the difficulty of obtaining these compounds in a crystalline form has hitherto prevented their composition from being investigated so accurately as their interesting nature requires.

"When the powder of algaroth is first thrown down, it forms a beautiful white precipitate; but if allowed to stand some time, or be collected on the filter, it not unfrequently changes in appearance, becomes granular, and assumes a yellowish-grey colour. In this state acid is separated from it by washing with water, but its whiteness is not restored. These grains sometimes possess a slight degree of lustre; they are minute crystals.

"If an acid chloride of antimony, prepared by digesting muriatic acid on the sulphuret or by any other process, be diluted with 20 or 30 times its volume of water, and set aside, the light white powder which at first falls gradually contracts in bulk, and at the end of two or three days has nearly all assumed the crystalline form. These crystals are grey, yellowish-grey, or, if the chloride has been prepared from the sulphuret and any sulphuretted hydrogen is present, of an orange-red colour. They exhibit occasionally a high degree of lustre, and, according to the measurement of Professor Miller of Cambridge, are oblique rectangular prisms, of which the terminal obtuse solid angles are replaced by planes. The crystals are generally microscopic, but from their brilliancy the faces are very distinctly observable. Only on two occasions have I observed them form radiated needles, brilliant, almost pure white, and half an inch in length; the method above described, however, seldom fails to give distinct crystals of a less size.

"When these crystals are washed with water, their brilliancy diminishes from the surface undergoing decomposition; they must therefore be collected and dried on bibulous paper without washing. That these constitute a definite compound there can be no doubt.

"At a temperature considerably above 212° Fahrenheit they lose no weight; further heated in a close vessel they decrepitate and give off white fumes of chloride of antimony; 31·86 grains heated to redness in a glass tube with twice its weight of dry carbonate of soda, and the gas given off made to pass over chloride of calcium, gave only ·12 of moisture = 0·38 per cent. This small quantity of moisture was obviously hygrometric either in the soda or the salt.

* Gmelin's Handbuch, I. p. 985. † Traité de Chimie, iii. p. 399.

‡ Turner's Chemistry, p. 697. [Mr. Phillips's analysis will be found in Phil. Mag. and Annals, N.S., vol. viii. p. 408.—Edit.]

"By the most careful procedure, however, it is difficult to obtain this compound free from a trace of chloride or oxide, which causes a small loss when heated to redness. By this process I obtained in three experiments 75·93, 76·506, 75·98, for the per-centage of metal. 3rd, Gently heated in an atmosphere of sulphuretted hydrogen, muriatic acid is disengaged, a little chloride of antimony is carried over with it, and the black sulphuret remains. By a cautious application of the heat, so as to prolong the process for two or three hours, the quantity of chloride volatilized may be very much diminished, so as to reduce the loss below a quarter per cent. Should any notable quantity of white fumes make their escape, they may be decomposed by causing them to pass through water, and the antimony afterwards collected in the state of sulphuret. In two experiments, I obtained by this method 76·558 and 76·6 for the amount of antimony. In these results I have most confidence; in both, however, there was a trifling loss, so that they ought to be rather under the truth.

"I endeavoured to determine the oxygen directly by heating the salt to redness with charcoal in a glass tube and collecting the carbonic acid evolved, but the approximation obtained was very rude. Reckoning the loss as oxygen, therefore, we have for the composition of the salt,

Chlorine	= 11·32	=	2·55	atoms.
Antimony	= 76·6	=	9·498	—
Oxygen	= 12·08	=	12·08	—
	100			

where the number of atoms of antimony is to this sum of those of chlorine and oxygen as 2 to 3·08, very nearly the relation which the electro-negative element bears to the metal in the oxide and first chloride of antimony.

"The atomic constitution which agrees most nearly with these numbers is the following:—

			By calculation.	By experiment.
	$\frac{3}{2}$ Cl	= 6·639	= 11·49	= 11·32
	Sb	= 8·064	= 76·72	= 76·6
4·5	Sb	= 36·290		
	$\frac{3}{2}$ O	= 6·75	= 11·79	= 12·08
		57·743	100	100

or two atoms of chloride are combined with 9 of oxide, the formula being $2\,(3\,Cl + 2\,Sb) + 9\,\ddot{Sb}$. The quantities of chlorine and antimony found by experiment are, as was to be expected, a little less than is indicated by theory, causing the amount of oxygen to appear something greater than it ought to be."—*Jameson's Edinburgh Philosophical Journal, No.* 35.

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LETTER FROM MR. WATKINS.

To Richard Phillips, Esq., F.R.S., &c.

My dear Sir,

I am fully aware that the pages of your Journal are far too valuable to be occupied by silly and consequently useless controversy; still I hope you will be able to favour me with a space, in your next number, for the following brief remarks.

In the last number of your Journal, page 231, I observe in the first paragraph of a long letter from Mr. William Sturgeon, that he has been pleased to indulge himself in using rather strong language against some one. I have accordingly inquired of this gentleman whether in that paragraph he alluded to me, and he has very *politely* answered me "Yes".

It therefore only remains for me in reply to his attack simply to repeat, what I have before stated, in a letter in your Journal, page 238, March number, that the phænomena which Mr. Sturgeon recorded as new in November 1834, were known and exhibited to some of the first philosophers of this country many months previously; and if Mr. Sturgeon doubt my assertion, I refer him without hesitation to any of the gentlemen mentioned in my letter above alluded to.

I remain, My dear Sir,
Yours faithfully,
Francis Watkins.

5 Charing Cross,
Sept. 10th, 1835.

ANALYSIS OF WOLFRAM.

In 1796 Vauquelin analysed wolfram, and in 1815 it was repeated by Berzelius; the results were:

	Vauquelin.	Berzelius.
Tungstic acid	67·00	74·666
Protoxide of manganese	6·25	5·640
Protoxide of iron	18·00	17·954
Silica	1·50	2·100
	92·7	100·360

Mr. Thomas Richardson has lately again analysed this mineral, and finds its composition to be:

Tungstic acid	73·60
Protoxide of iron	11·20
Protoxide of manganese	15·75
	100·55

The difference between this and the former analysis would lead to the opinion that they were different species.—*Thomson's Records of Science*, vol. i. p. 452.

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London, and by Mr. VEALL, at Boston.

Days of Month. 1835.	Barometer. London. Max.	Barometer. London. Min.	Barometer. Boston. 8½ A.M.	Thermometer. London. Max.	Thermometer. London. Min.	Thermometer. Bost. 8½ A.M.	Wind. Lond.	Wind. Bost.	Rain. Lond.	Rain. Bost.
Aug. 1	30·123	30·031	29·50	84	50	64	SE.	calm	...	...
2	30·003	29·942	29·38	80	54	66·5	E.	calm	...	...
3	30·039	30·019	29·50	74	44	63	NE.	E.	...	...
4	30·029	29·983	29·45	81	44	65	E.	NE.	...	...
5	30·025	29·978	29·35	84	59	67	SE.	W.	0·01	...
6	30·075	30·017	29·33	72	60	66·5	SW.	W.	·01	...
7	30·131	29·959	29·30	75	42	64	SW.	NW.	·01	...
○ 8	30·330	30·250	29·65	76	41	55	N.	NW.	...	...
9	30·355	30·328	29·77	89	45	59	NE.	calm	...	...
10	30·302	30·202	29·62	91	69	70	S.	calm	...	...
11	30·128	29·903	29·40	92	55	71·5	S.	calm	...	...
12	30·023	29·930	29·15	86	57	67	W.	W.	...	...
13	30·168	30·064	29·46	78	48	60	NE.	NE.	...	...
14	30·212	30·202	29·64	76	42	64	NE.	calm	...	...
15	30·182	30·148	29·56	82	55	64	NW.	calm	...	...
☾ 16	30·207	30·142	29·46	82	56	68	NW.	NW.	...	...
17	30·285	30·240	29·57	77	56	68	NW.	calm	...	...
18	30·269	30·237	29·68	81	57	67	E.	E.	...	...
19	30·206	30·061	29·56	84	56	68	E.	NE.	...	...
20	29·944	29·674	29·33	87	56	69	E.	calm	...	...
21	29·643	29·522	28·98	88	57	68	S.	calm	...	...
22	29·690	29·554	28·80	80	49	65·5	S.	calm	...	0·10
23	29·758	29·727	29·07	76	55	63	SW.	W.	·02	...
● 24	29·571	29·534	29·00	78	50	65	S.	E.	·09	·40
25	29·673	29·570	29·00	73	54	59	W.	NW.	·04	...
26	29·696	29·672	29·64	71	45	57·5	W.	N.	...	·13
27	29·820	29·734	29·16	73	47	59·5	E.	NW.	...	·20
28	30·063	29·986	29·53	73	42	63	E.	E.	...	·01
29	30·055	30·024	29·57	79	41	61·5	E.	E.	...	...
30	30·060	30·058	29·50	79	42	62	E.	E.	...	...
☽ 31	30·088	30·051	29·57	77	42	59	E.	NE.	...	...
	30·355	29·522	29·38	92	41	64·1			0·18	0·84

Remarks.

London. — August 1. Hot and dry. 2. Overcast: fine: lightning at night. 3. Dry haze: fine. 4. Fine. 5—7. Cloudy and fine: very slight rain at nights. 8, 9. Fine but dry. 10, 11. Hot and dry: thermometer above 90° in the shade. 12—16. Fine. 17—20. Dry haze in the mornings: hot and dry. 21, 22. Cloudy and fine. 23. Slight showers. 24. Showery. 25. Cloudy and fine. 26. Overcast. 27. Hazy: fine. 28—30. Slight haze: fine. 31. Clear and fine.—This month has been one of almost continued dryness, the amount of rain being scarcely one fifth of an inch. Grass has been greatly scorched; and trees in many instances have dropped their foliage. Were it not the case that the continuation of the cause tends to counteract the effect, the destruction in vegetation from protracted drought would be immense; but the foliage, as moisture is denied, breaks out into a less and less expansive form, and consequently occasions much less waste by evaporation.

Boston.—Aug. 1. Fine. 2. Cloudy. 3—12. Fine. 13. Cloudy. 14. Fine. 15. Cloudy: a few drops of rain P.M. 16. Fine. 17. Cloudy. 18—20. Fine. 21. Fine: thunder and lightning with rain P.M. 22. Cloudy: rain early A.M. 23. Fine. 24. Cloudy: thunder and lightning with rain A.M. 25. Cloudy: rain P.M. 26. Cloudy: rain A.M. and P.M. 27. Cloudy: rain A.M. 28. Fine: rain A.M. 29, 30. Fine. 31. Cloudy.

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XXXVII. *Reply to* Dr. John Davy's "*Remarks on certain Statements of* Mr. Faraday *contained in his 'Researches in Electricity.'*" *By* MICHAEL FARADAY, *D.C.L. F.R.S. &c. &c.**

To Richard Phillips, Esq.

MY DEAR PHILLIPS,

YOU know as well as most persons how great my dislike is to controversy, but you also know that upon some rare occasions I have been driven into it; an occasion of this kind constrains me at present to ask a favour of you. On the 22nd of January of this year, two papers were read at the Royal Society, the first entitled "Remarks on certain Statements of Mr. Faraday contained in the Fourth and Fifth Series of his Experimental Researches in Electricity," by Dr. Davy; the second, "A Note in reference to the preceding Observations," by myself. These the Royal Society did not think fit to publish in the Philosophical Transactions, but the notice of the readings appears in the 'Proceedings' of the Society, No. 19, and in your Philosophical Magazine, April 1835, page 301.

I now find that Dr. Davy has published his paper in the last number of the Edinburgh New Philosophical Journal, p. 317. I was in hopes that if that paper appeared in print mine might have immediately followed it; and meeting Dr. Davy in the Royal Institution in May last, asked him to do me the favour to allow that to be the case: this, I presume for good reasons, (which, however, I do not understand,) he declined. I am thus placed in a difficult position; for, however willing Pro-

* See Jameson's Edinburgh New Philosophical Journal, October 1835, p. 317—325.

fessor Jameson, the learned Editor of the Edinburgh Journal, may be to act impartially, and give me the same opportunity of publication he has given Dr. Davy, he cannot do so before the lapse of three months. Under these circumstances, and with the old adage before my eyes that "delays are dangerous," may I beg you to insert this letter and my paper in the next Number of the Philosophical Magazine? and I may still, perhaps, be indebted to the kindness of Professor Jameson for its insertion in the next number of the Journal in which Dr. Davy's "Remarks" have appeared.

I am, my dear Sir, most truly yours,

Royal Institution, Oct. 10, 1835. M. FARADAY.

The secretary of the Royal Society having mentioned to me the preceding paper, I requested a sight of it, that I might as soon as possible correct any error in the papers to which it referred, and of which it might make me conscious; and having read it, I am induced to hope the present note may accompany Dr. Davy's observations.

I do not know that I have any right to suppose Dr. Davy generally does not understand me in my papers, and yet something of this kind must have occurred; for instance, the new law of conduction referred to in my Fourth Series* is not even now evident to him, and therefore I think I cannot have erred in supposing Sir Humphry Davy unacquainted with it. The *law* is, that *all substances* decomposable by the pile are in the fluid state conductors, and in the solid state nonconductors, of the electricity of the voltaic battery (393. 394. 404. 407. 413. 505. 676. 679. 697., &c.†). The more careful examination of this law in other parts of my printed Researches shows that no bodies but electrolytes have this relation to heat and electricity, the few exceptions which seem to occur being probably only apparent (690. &c.†). That the title of *law*, therefore, is merited, and that this law was not known to Sir Humphry Davy, are, I think, justifiable conclusions, notwithstanding Dr. Davy's remarks. As to Priestley's results with the electric machine, they really have nothing to do with the matter.

I have said that Sir Humphry Davy spoke in general terms. "The mode of action by which the effects take place is stated very generally, so generally indeed that probably a dozen precise schemes of electro-chemical action might be drawn up differing essentially from each other, yet all agreeing with the

[* An abstract of Mr. Faraday's Fourth Series will be found in Lond. and Edinb. Phil. Mag., vol. iii. pp. 449, 450.—EDIT.]

[† The paragraphs here referred to belong to Mr. Faraday's Seventh Series, and will be found reprinted in Lond. and Edinb. Phil. Mag., vol. v. p. 166—169.—EDIT.]

statement there given (482.)." In this and other parts of what I have written (483. 484. *), which Dr. Davy quotes, he thinks that I have been deficient in doing justice, or in stating Sir Humphry Davy's "hypotheses" correctly.

Dr. Davy for my word "general" substitutes "vagueness". I used *general* in contradistinction to *particular*, and I fear that vagueness cannot with propriety stand in the same relation. I am sure that if Sir Humphry Davy were alive, he would approve of the word I have used; for what is the case? Nearly thirty years ago he put forth a *general* view of electro-chemical action, which, as a general view, has stood the test to this day; and I have had the high pleasure of seeing the Royal Society approve and print in its Transactions of last year, a laborious paper of mine in support and confirmation of that view (1834. Part ii. page 448.†). But that it is not a particular account is shown, not merely by the manner in which Sir Humphry Davy wrote, but by the sense of his expressions, for, *as Dr. Davy says*, "he attached to them no undue importance, believing that our philosophical systems are very imperfect, and confident that they must change more or less with the advancement of knowledge‡;" and what have I done but helped with many others to advance what he began; to support what he founded?

That I am not the only one, as Dr. Davy seems to think, who cannot make out the precise (or, I would rather say, the particular) meaning of Sir Humphry Davy in some parts of his papers may be shown by a reference to Dr. Turner's excellent Elements of Chemistry, where, at page 167 of the fifth edition, the author says: "The views of Davy, both in his original essay and in his subsequent explanation (Philosophical Transactions 1826), were so *generally and obscurely* expressed that chemists have never fully agreed, as to some points of the doctrine, about his real meaning. *If* he meant that a particle of free oxygen or free chlorine is in a negatively excited state, then his opinion is contrary to the fact, that neither of these gases affect an electrometer," &c. &c. Having similar feelings, I thought that I was doing Sir Humphry Davy far more justice in considering his expressions as general, and not particular, except where they were evidently intended to be precise, as in the cases which I formerly quoted (483. 484.).§

[* These paragraphs belong to the Fifth Series, noticed in Lond. and Edinb. Phil. Mag., vol. iii. p. 460.—EDIT.]

[† See Lond. and Edinb. Phil. Mag., vol. vi. p. 181.—EDIT.]

‡ Phil. Trans. 1826, p. 390. Edinb. New Phil. Journ., Oct. 1835, p. 323.

§ I may be allowed to quote in a note a passage from one of Mr. Prideaux's papers, of the date of March 1833; I was not aware of it when

Again, Dr. Davy says, "What can be more clear than this; that my brother did not consider water as essential to the formation of a voltaic combination?" &c. If this be so clear, how happens it that Mr. Brande, in the last edition of his Manual, vol. i. p. 97, says that "Sir Humphry Davy further remarks that *there are no fluids, except such as contain water,* which are capable of being made the medium of connexion between the metals of the voltaic apparatus;" and Mr. Brande's observation is, "This, however, appears to me to admit of doubt."? How happens it also that Dr. Ure, in giving his eloquent account of Sir Humphry Davy's discoveries*, uses the very same words as those I have quoted from Mr. Brande, adding, "It is probable that the power of water to receive double polarities and to evolve oxygen and hydrogen is *necessary* to the constant operation of the connected battery."? I ought, perhaps, rather to ask, How could Sir Humphry Davy use such words, and mean what Dr. Davy wishes to be considered as his meaning? Why, *there can be no doubt that if I had proved that water was the only substance that could perform these duties, Dr. Davy would have claimed the discovery for his brother.*

As I cannot impute to Dr. Davy *the intention of doing injustice*, the only conclusion I can come to is that the language of Sir Humphry Davy is obscure even to his brother, who thinks it perfectly clear; so obscure, indeed, as to leave on his mind the conviction of a meaning the very reverse of that which it bears to Mr. Brande and Dr. Ure. Thus Dr. Davy puts his seal to the truth of Dr. Turner's observation† by the very act of denying it.

What makes the matter still more remarkable is, that Dr. Davy charges it upon me as a fault, that I, and *I alone*, have said what he denies in words, but proves in fact; whereas *I have not said* it, and others have.

If Sir Humphry Davy's meaning is thus obscure to his brother, I have no right to expect that mine should have been rightly taken; and therefore it is that I suspect, as I said be-

I wrote in answer to Dr. Davy. Mr. Prideaux says, "Sir Humphry Davy's theory assumes that 'chemical and electrical attractions are produced by the same cause; acting in one case on particles, in the other on masses: and the same property, under different modifications, is the cause of all the phænomena exhibited by different voltaic combinations.' A view so comprehensive, embracing every modification of chemical as well as electrical action, seems to include the other two, *and every one that has been or can be attempted* on the subject. But what it gains in extent it wants in distinctness." Lond. and Edinb. Phil. Mag., vol. ii. p. 215.

* Chemical Dictionary, *art.* Electricity.

† And to that of Mr. Prideaux's also.

fore, that Dr. Davy generally does not understand me in my papers.

That "probably a dozen precise schemes of *electro-chemical action* might be drawn up "differing from each other, but all agreeing with Sir Humphry Davy's general statement," is no exaggeration. I have in the very paper which is the subject of Dr. Davy's remarks quoted six: 1. that of Grotthus (481.); 2. of Sir Humphry Davy himself (482.); 3. of Riffault and Chompré (485.); 4. of Biot (486.); 5. of De la Rive (489.); and 6. my own (518. &c.). These refer to modes of decomposition only; but as I spoke in the passage above quoted of "electro-chemical action" in reference to chemical effects and their cause generally, I may now quote other particular views. Volta, Pfaff, Marianini, &c. consider the electricity of the voltaic pile due to contact alone. Davy considered it as excited by contact, but continued by chemical action. Wollaston, De la Rive, Parrot, Pouillet, &c. considered it as of purely chemical origin. Davy, I believe, considered the particles of matter as possessing an inherent electrical state to which their chemical properties were due; but I am not sure of his meaning in this respect. Berzelius, according to Turner, views them as being naturally indifferent, but having a natural appetency to assume one state in preference to another*, and this appears to be the theory of M. Fechner also†. Again, electro-chemical phænomena have been hypothetically referred to vibrations by Pictet, Savary, myself, and others. Now, all these views differ one from another; and there are, I think, a dozen of them, and it is very likely that a dozen more exist in print if I knew where to look for them; yet I have no doubt that if any one of those above could be proved by a sudden discovery to be the right one, it would be included by Dr. Davy, and, as far as I can perceive, by myself also, in Sir Humphry Davy's general statement. What ground is there, therefore, for Dr. Davy's remarks on this point?

In reference to another part of Dr. Davy's observations I may remark, that I was by no means in the same relation as to scientific communication with Sir Humphry Davy after I became a fellow of the Royal Society in 1824, as before that period, and of this I presume Dr. Davy is aware. But if it had been otherwise, I do not see that I could have gone to a fitter source for information than to his printed papers. Whenever I have ventured to follow in the path which Sir Humphry Davy has trod, I have done so with respect and with the highest admiration of his talents, and nothing gave

* Turner's Elements, Fifth Edit., p. 167.

† Quarterly Journal of Science, vol. xxvi. p. 428.

me more pleasure in relation to my last published paper, the Eighth Series*, than the thought that whilst I was helping to elucidate a *still obscure* branch of science, I was able to support the views advanced twenty-eight years ago, and for the first time, by our great philosopher.

I have such extreme dislike to controversy that I shall not prolong these remarks, and regret much that I have been obliged to make them. I am not conscious of having been unjust to Sir Humphry Davy, to whom I am anxious to give all due honour; but, on the other hand, I feel anxious lest Dr. Davy should inadvertently be doing injury to his brother by attaching a meaning, sometimes of particularity and sometimes of extension, to his words which I am sure he would never himself have claimed, but which, on the contrary, I feel he has disavowed in saying "that our philosophical systems are very imperfect," and in expressing his confidence "that they *must change more or less with the advancement of science.*" On these points, however, neither Dr. Davy nor myself can now assume to be judges, since with respect to them he has made us both partisans. Dr. Davy has not made me aware of anything that I need change; and I am quite willing to leave the matter as it stands in the printed papers before scientific men, with only this request, which I am sure beforehand will be granted, that such parts of Sir Humphry Davy's papers and my own as relate to the subject in question, be considered both as to their letter and spirit before any conclusion be drawn.

Royal Institution, January 9, 1835.

XXXVIII. *Note by* M. Ampère *on Heat and Light considered as the Results of Vibratory Motion.*†

THANKS to the labours of Young, Arago, and Fresnel, it is now demonstrated that light is produced by the vibrations of a fluid diffused throughout space, and which has been called æther. Radiant heat, which follows in its propagation the same laws, may be explained in the same manner. But when heat is propagated from the most highly heated part of a body, to another which is less heated, the laws of its transmission are entirely different: instead of a vibratory motion propagated in undulations or waves in such a manner that every wave leaves at rest the fluid which it sets in motion at the instant of its passage, we have a motion propagated gradually in such a manner that the part which originally was the hottest, and consequently the most agitated (explaining

[* Reprinted in Lond. and Edinb. Phil. Mag., vol. vi. p. 34 *et seq.*—Edit.]

† From the *Annales de Chimie et de Physique,* tome lviii. p. 432—444.

the phænomena of heat by the theory of vibratory motions), although losing heat by degrees, preserves, however, more than the parts to which it is communicating heat. Hence an objection arises against the theory of the transmission of heat by vibratory motion*.

In a note inserted in 1832 in the 49th volume of the *Bibliothèque Universelle* of Geneva, page 225, I endeavoured to answer this objection by showing from which sort of motion arise the phænomena of which I have spoken. I intend now again to make public my ideas upon the subject, and adding to them some more extended developments.

The principle I have announced rests on the distinction I established long since between *particles*, *molecules*, and *atoms*. I call a *particle* an infinitely small part of a body of the same nature with it, so that a particle of a solid body is solid, a particle of a liquid body liquid, and of a gas aëriform.

Particles are composed of molecules held at a distance from each other; first, by what at that distance remains of the attractive and repulsive forces proper to the atoms; 2ndly, by the repulsion established between them by the vibratory motion of the interposed æther; 3rdly, by attraction in the direct ratio of the masses, and in the inverse ratio of the square of the distances. The term *molecules* I give to an assemblage of atoms held at a distance from each other by the attractive and repulsive forces proper to every atom, forces which I admit to be so superior to the preceding that those may be considered relatively as almost insensible. What I call *atoms*, are the material points from which these attractive and repulsive forces emanate.

From this definition of molecules and atoms it follows that the molecule is essentially solid, whether the body to which it belongs be solid, liquid, or gaseous; that the molecules have necessarily the form of a polyhedron, of which the atoms, or a certain number of them at least, occupy the summits. These polyhedral forms are those called by crystallographers primitive forms.

I admit that in the transition of bodies from the liquid to the gaseous state, and reciprocally, the molecules in passing from one of these states of equilibrium between forces which determine their distance, to another state of equilibrium between the same forces, merely recede from or approach one another; but I think that in passing from the liquid to the solid state, two

* However, as a body exposed to the rays of the sun is heated at first on that part on which the rays fall, and that heat is gradually transmitted to the remainder, it is impossible to admit that the light and heat of the solar rays consist in vibrations, without admitting also that the heat transmitted in the interior of a body is equally produced by vibratory motions.

or more of these molecules unite to form more compound ones.

Mechanical forces can only separate the particles: the force resulting from the vibration of the atoms can separate the more compound molecule of a solid substance into more simple ones, such as those which exist in a liquid or in a gas. Chemical forces alone can separate ulteriorly these last molecules. For example, in the detonation of the mixture of one volume of oxygen with two of hydrogen, from which result two volumes of aqueous vapour, reduced to the original temperature, every molecule of oxygen is divided into two, and the atoms of each of these halves unite to the atoms of a molecule of hydrogen to form a molecule of water.

This division of molecules by chemical forces results from a principle which I established in a memoir printed in 1814, in the *Annales des Mines*, namely, that in equal volumes of any gas or vapour whatever, under the same pressure and at the same temperature, are contained the same number of molecules.

With regard to atoms, the only property which I think can with certainty be attributed to them is that of being absolutely indivisible, so that although space be infinitely divisible, matter cannot be so, for when the division is conceived to have arrived at the atoms, every further division would of necessity take place in the intervening empty spaces*. Entertaining such views of this subject, I distinguish molecular from atomic vibrations. In the first the molecules vibrate collectively in approaching to and receding from one another alternately; and whether they so vibrate or are at rest, the atoms of every molecule can, and, in fact, always do, vibrate in alternately approaching and receding from one another, without ceasing to belong to the same molecule. These last I call atomic vibrations.

It is to molecular vibrations, and their propagation through ambient mediums, that I attribute all the phænomena of sound; to atomic vibrations, and their propagation through the æther, I attribute all the phænomena of heat and light. Vibratory motion not being possible except around a state of stable equilibrium between opposed forces, the atomic vibrations necessarily suppose the existence of a repulsive force in equilibrium with an attractive force, these two forces acting at the same time between two atoms, so as to allow the possibility of a stable equilibrium between these two forces; which circumstance will necessarily cause the repulsive force to increase or decrease with greater rapidity than the attrac-

[* On this subject see Phil. Mag., first series, vol. lxii. p. 360, and vol. lxiii. p. 372.—Edit.]

tive force when the distance varies. Finally, these two forces may be considered as one, the mathematical expression of which should contain two terms with contrary signs, each corresponding to one of the forces in question.

Now, it is clear that if we admit the phænomena of heat to be produced by vibrations, it is a contradition to attribute to heat the repulsive force of the atoms requisite to enable them to vibrate.

In order to form a clear idea of the manner in which heat is propagated according to the several laws, 1st, when it moves within the substance of a body, and 2ndly, when it exists in the state of radiant heat, it is necessary to observe at starting that in the latter case it cannot be distinguished from light, for light is nothing else than radiant heat having become capable of passing through the humours of the eye, because the frequency and intensity of the vibrations which constitute it are then sufficiently powerful to be transmitted through these humours*. It will next be necessary to compare these two sorts of propagations to the two modes in which sound is propagated. As a preliminary we will describe these two modes of propagation.

Suppose a diapason set in vibration, and endeavour to define, first, what we ought to understand by the *vis viva* of its vibratory motion. We obtain this *vis viva* by the summation of the products of the masses of all its molecules by the squares of their velocities at a given moment, adding double the integral of the sum of the products of the forces multiplied by the differentials of the spaces described, in the direction of these forces, by each molecule; this integral, which depends only on the relative position of the molecules, being taken in such a manner as to be null in the position of equilibrium around which the vibration takes place†.

* Experience proves that radiant heat, up to the temperature which causes the bodies from which it proceeds to be visible in the dark, cannot pass through water, either in a fluid or a solid state, and that, on the contrary, as soon as it attains the temperature of incandescence, it acquires the power of traversing that medium. To explain this coincidence of two facts which appear independent of one another, I set out from the principle that under a certain degree of intensity and frequency of the vibratory motions which constitute radiant heat, these vibrations cannot be propagated through water, and that above that degree they are propagated through it to a greater or a smaller amount. It is clear in the first case that there can be no sensation of light, because the vibrations cannot be propagated through the humours of the eye, nor, consequently, reach the retina; and in the second, that the bodies must become visible in the dark by the rays which have then the power to penetrate the same humours.

† The integral taken in this manner is positive in all positions near those of equilibrium; and that is the character of stable equilibrium around which alone vibratory motions can take place. From this it fol-

Let us see then what takes place in the diapason in the following cases.

1. If the diapason be *in vacuo*, the vibratory motion is indefinitely continued, and the sum of the *vires vivæ*, whether implicit or explicit, remains constant.

2. If the diapason be in a fluid the density of which is less than its own, every entire vibration of the diapason, that is to say, its motion within the compass of two complete returns to the same position with velocities in the same direction, will produce in the fluid an undulation of a determined thickness, which traverses it, according to the known laws of the propagation of sound, leaving at rest the part it has traversed, excepting the motion which the following wave will produce if the diapason continues vibrating. At every vibration, the *vis viva* of the diapason will have lost all the *vis viva* which passes into the wave, so that the different successive losses which the diapason will sustain will gradually diminish with the intensity of the waves which it produces.

3. If the diapason be in a fluid of the same density and elasticity with itself, it will be deprived of all motion at the first vibration, and the whole of its *vis viva* will pass into the only wave which it will propagate around itself.

4. If there be in an indefinite medium any number whatever of diapasons in unison, of which a single diapason or a group of neighbouring diapasons are in vibration, the waves produced in the medium, which we suppose to be of a density much inferior to that of the diapasons, in meeting those of the diapasons which were at rest, will gradually communicate to them motions so much the less the greater their distance is from the vibrating group, the *vis viva* of that part of the waves which meets no diapason being lost to the system. But in proportion as the diapasons which first were at rest are set in motion, they will produce new waves, a part of the *vis viva* of which will return to the first group, returning to it a smaller amount of *vis viva* than it receives, as by virtue of these mutual exchanges the *vis viva* of their vibrations cannot increase but in proportion as it becomes inferior to that of the group

lows that, the *vis viva* being the same, the sum of the products of the masses by the squares of the velocities would become a maximum in the case in which all the molecules would pass at once through the position of equilibrium; for it would be when the other part, always positive, of what we have called *vis viva*, is 0. I term the first part of the *vis viva* resulting from the masses multiplied by the squares of their velocities *explicit vis viva* (*force vive explicite*), and the double of the integral designated above *implicit vis viva*. In a system which is retained at rest out of the position of equilibrium there is a positive implicit force equal to double the value of that integral in the position in which the body is retained.

originally vibrating. The *vis viva* of the system of all the diapasons will by degrees diminish indefinitely, in consequence of the waves which the medium propagates out of the system; unless we suppose it to be inclosed within a sphere (*une enceinte*) of diapasons kept in a state of vibration with a constant *vis viva*, which we suppose, for example, to be inferior to that of the part of the system originally vibrating. In that case, the *vis viva* of that part and of the rest of the system tend to approach indefinitely that of the diapasons inclosed within the sphere, without ever, mathematically speaking, being capable of attaining it, because the diapasons within that sphere (*enceinte*), which have a *vis viva* superior to its own, will lose a part of it by the presence of the surrounding sphere communicating to it more than they receive; and that of those which have less than the sphere will, on the contrary, receive less than they send out.

If we consider the diapasons placed in a cylindrical envelope of very small diameter to observe their propagation in one dimension only, or placed between two planes to observe the propagation in two dimensions, or merely placed in space, we may suppose in the first case the length of the cylinder occupied by these diapasons to be divided into a number of slices equal in length and parallel to the bases of the cylinder; in the second, that the space comprised between the two parallel planes is divided into circular zones of equal magnitude, the centre of which is in that part, supposed to be very little, where the diapasons had originally vibrated; in the third case, in which the space surrounding the same part is divided into spherical layers of equal thickness; and we shall discover the *vis viva* in all those cases to be transmitted from the diapasons of a division of a zone, or of a spherical layer, to the diapasons of the consecutive slices, zones, or spherical layers. This transmitted quantity will be found null if their *vis viva* was equal; we may therefore suppose, as a first approximation, that the quantity of *vis viva* transmitted is proportional to the difference of the *vires vivæ* of the two consecutive groups of diapasons. We then find necessarily, with regard to the distribution of the *vis viva* in the diapasons, the same equations as those found by Fourier, for the distribution of heat in the three corresponding cases, setting out from the same hypothesis, that the temperature or transmitted heat, which here represents the transmitted *vis viva*, is proportional to the difference of the respective values of the temperatures. And supposing we apply to the transmission of the *vis viva* between the diapasons other laws,—for example, the laws which M. Libri or M. Babinet have pro-

posed for the transmission of heat,—it is evident that we shall find for the transmission of this force in the systems of diapasons the same equations at which those philosophers have arrived for temperature agreeably to their respective hypotheses.

Let us observe here, that so long as we consider the diapasons as having sensibly but one single dimension, we are obliged to subject them to the condition of being capable of vibrating in unison; but as M. Savart has demonstrated that vibrating forces of two dimensions, and with greater reason vibrating bodies of three dimensions, are susceptible, by gradual changes in the nodal lines, to take the unison of any vibrating body whatever, it suffices here to substitute, in everything that precedes, the name of diapason for that of a vibrating plate or solid, in order to find true everything we have been saying, without any condition relative to the form or dimension of the bodies.

Let us now apply this to the molecules of a heated body, considering these molecules as so many diminutive solids susceptible of vibrating independently of one another, and of communicating gradually parts of the *vis viva* of their motion to the surrounding æther, producing in it an undulation or wave at every vibration, precisely as a diapason communicates one part of the *vis viva* of its vibratory motion to the surrounding air; and let us admit that it is only through the medium of this æther that a neighbouring molecule, which has a less intense vibratory motion, increases gradually its *vis viva* in proportion as this force is inferior to the force of the first molecule; it is evident that we shall find, by the distribution of the *vis viva* among the different molecules, precisely the same equations as those which have been given for the distribution of heat, according to the different hypotheses respecting the manner in which the *vis viva* transmitted from one molecule to another depends upon the difference in their temperatures.

We find manifestly the same result by considering the subject as we have just enunciated it, according to the system of emission, or according to that of vibrations, substituting for the quantity of caloric in the first system, the *vis viva* of the vibratory motions of the molecules in the second. It was in order to render the analogy between the propagation of heat in bodies and that of sonorous vibrations from solid to solid, through the medium of the air, more easy of comprehension that I supposed in this explanation that the molecules of bodies do not transmit their vibratory motions one to another, except through the medium of the æther: but I think that the molecules can also transmit heat one to another [imme-

diately]; that in the change of form of a molecule, whatever may remain, at the distance at which it is situate from the neighbouring molecule, of the attractive and repulsive forces of the atoms of which the two molecules are composed, is susceptible of experiencing any changes which tend to make the atoms of the second molecule vibrate. But this manner of considering the subject requiring calculations which I have not made, I have not thought proper to insist on the development of the consequences of this idea. My object in these considerations is only to demonstrate how the vibrations by which heat is propagated in bodies may follow a law entirely different from that of the vibrations of sound, of light and of radiant heat, these latter vibrations being propagated by undulations which leave at rest the part of the vibrating body where they have passed, without a trace remaining of the passage, while the former are made gradually, and from one object to the nearest, and in such a manner that the vibrations of those parts which are nearer to the source of heat remain always superior in intensity to the vibrations more distant, by a quantity which truly diminishes continually and by degrees, but that, mathematically speaking, would only become nothing after an infinite lapse of time.

XXXIX. *Note relative to the Polarization of Heat.* By J. D. Forbes, *Esq., F.R.SS. L. & E., Professor of Natural Philosophy in the University of Edinburgh.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

By the Report of Professor Powell's communication to the British Association at Dublin on the subject of radiant heat, which appears in your Journal for last month (p. 296), it seems to me that a doubt is thrown upon one case of the polarization of heat which I have described,—a doubt which I will endeavour to remove with as much brevity as possible.

The experiment is this. Two bundles of mica plates are prepared as I have described in my paper*, and each is attached by wax to a *sole* of wood, so as to sustain it at the polarizing angle when the sole lies flat. When two such bundles are interposed as shown on page 351 (fig. 1.), between the source of heat S, and the thermo-electric pile P, more heat reaches the pile than when one of the bundles is raised on edge so as to be crossed relatively to the other, as in fig. 2.

It may be argued, that in these two positions the plates

[* See Lond. and Edinb. Phil. Mag., vol. vi. p. 210.—Edit.]

not being symmetrically arranged, more heat may possibly reach the pile *by conduction* in one case than in the other, without any reference to polarization.

The question is not, however, as has been stated, "purely mathematical." It must be shown that the alleged effect is not merely of the *kind* required, but that its *amount* is such as to produce the observed variation; and since the temperature acquired by conduction is unknown, and can only be assigned by its *effect*, the question, on the contrary, is purely experimental.

Had the objectors taken the trouble to try the experiment, the difficulty would never have been urged. I have never denied the interference of the effects of conduction; I have contented myself with the assurance that they were of an order sufficiently insignificant not to *produce* the effects I observed, which in almost every case, on the contrary, they tended to diminish, as I have shown in my memoir; and for that reason I gave my numerical results as approximations only. I never thought of entering into the fatiguing detail which would have been necessary for explaining fully my grounds of confidence in the experimental results at which I arrived, which were chiefly such as must immediately offer themselves to those who would attempt to repeat them, which I naturally considered as preliminary to any criticism upon their soundness.

Melloni has shown that in his experiments the effect of the warmth, or conducted heat, of the interposed plate was always or almost always insensible. I might argue that the effect of the still smaller quantity of heat which could reach the plate B, derived from that already so small in A (fig. 1.), must be insignificant; and therefore the variation of this quantity, owing to the relative change of position in question, must be almost infinitely small even in my experiments, in which the approximation of the source of heat was much greater than in those of Melloni. And this argument would be incontrovertible to any one who had tried the experiments, and who was capable of weighing quantitative evidence. But I have a much more direct reply.

The mathematical distinction which Professor Powell and Mr. Murphy established (and justly) between the cases of fig. 1. and fig. 2. must be expressed by the difference of certain integrals depending on the distances of the elements of the plates from one another in the two cases. But what will be said if I destroy entirely this distinction; or give it an opposite character, by pushing the plates nearer to or further from one another in one position than in the other; or if, in-

stead of making the plates parallel, as in fig. 1., I make them incline opposite ways, as in fig. 3., and still get the same results, whilst in the intermediate position of fig. 2. *forty per cent.* of the heat which reached the pile in the other cases is stopped, and that *instantly*, without any appearance of that gradual process which conduction implies? I imagine that those most urgent for mathematical precision will perceive that the effects of conduction above alluded to must be quantities far below the powers of our most delicate instruments to measure, and at all events of an order quite inferior to the great and striking effects I have described, and which can only be due to a peculiar influence of the plates upon the freely transmitted ray.

Fig. 1.

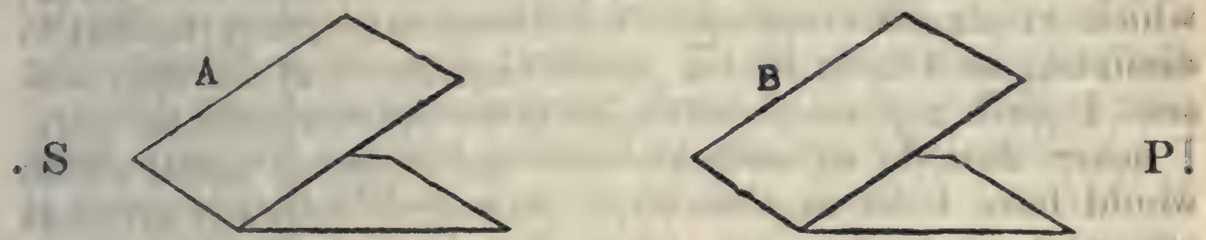


Fig. 2.

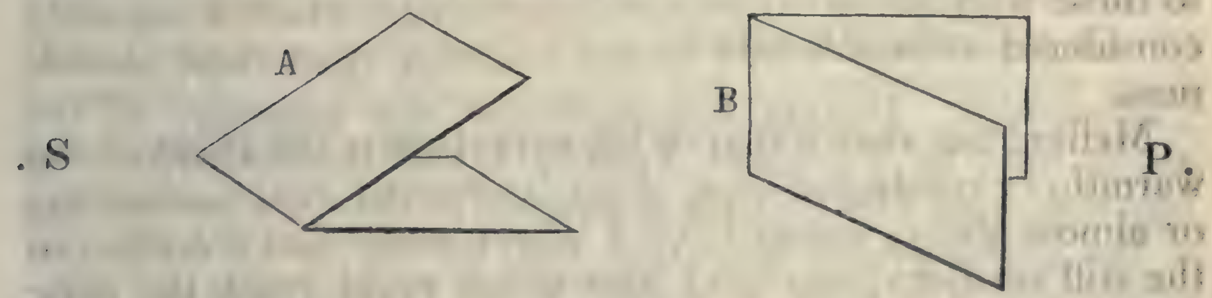
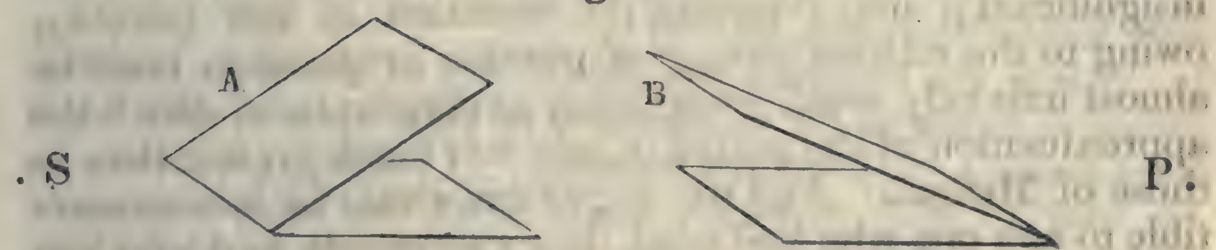


Fig. 3.



I speak with the more confidence on this point because the objection in question is not a new one. It was urged to me several months ago in Paris by a French philosopher of great eminence, who, however, had not had recourse to the only fair test, namely, an experimental one. Unprovided with any of the apparatus I had employed, I hastily and rudely prepared bundles of mica for the purpose of demonstrating the independence of the result upon any nice adjustment of the distances or relations of the plates, and I showed these experiments to MM. Melloni and Libri, whose names are im-

perishably connected with the science of heat, the one in its experimental, the other in its most recondite mathematical, department. In these experiments no less than *fifty per cent.* of the whole heat incident upon the pile in the circumstances of figg. 1. and 3. was polarized, that is, stopped, in the position of the plates in fig. 2.: the effect on the galvanometer was instantaneous, and the relative positions and distances of the plates were varied in every possible way without sensibly affecting even the quantitative measure of the results.

It would be easy to devise a vast variety of modes of operating which would equally annihilate the mathematical objection; at present, being anxious to confine myself to the narrowest possible limits, I will only add that in the beautiful and admirably marked phænomena of *depolarization* by a uniform mica plate introduced between the plates A and B, and perpendicularly to the incident ray, there subsists the most perfect symmetry which the mathematician can desire.

I have had the pleasure of learning within a few days, from M. Melloni himself, that he is now satisfied that heat is polarized by tourmaline, which was that of all my experiments in which conduction produced the most troublesome effects.

London, October 5, 1835.

Postscript. Having communicated the substance of the above note to Professor Powell, I had the satisfaction of learning from him that the statement in the October number of the Philosophical Magazine had led me to an erroneous apprehension of what he stated, at the Dublin meeting, relative to the polarization experiment. In relation to the question discussed above, he says:

"Allow me to assure you that if any of the reports represented it as urged for an *objection* to your results, they must have entirely mistaken my meaning. I proposed it merely as an *abstract problem* which would affect the *small correction* which (if the heat were intense enough) should be applied for the secondary radiation; but I have no doubt it is quite insensible."

I suffer the statement which I have made above, relative to this experiment, to remain, because it may serve as a general answer to a natural enough *primâ facie* objection; whilst it gives me the highest pleasure to be able to quote Professor Powell's opinion in support of my views instead of the reverse.

Edinburgh, October 21, 1835.

XL. *Some Remarks upon the Crag Formation of Norfolk and Suffolk.* By SAMUEL WOODWARD, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

PERMIT me, through the medium of your Journal, to offer a few remarks on a paper by Mr. Charlesworth inserted in your Number for August, p. 81.

Feeling a kindred spirit with Mr. Charlesworth in bringing these interesting deposits into notice, and moreover a desire that truth should prevail, I do, in the true spirit of scientific inquiry, state that my views of the subject do not square with those of that gentleman.

In the first place, his red crag is decidedly diluvium or disrupted crag, with an admixture of the oxide of iron, precisely similar to what we witness in the gravel of Norfolk. We have also in this county immense beds of transported crag shells, in the cliffs south and north of Yarmouth, and at Cromer, but with this difference,—they are imbedded in sand*.

Secondly, his term 'coralline crag' is not appropriate, as it leads us to suppose that it is composed of corallines, when, in fact, there are none in the Ramsholt bed, which is chiefly adverted to†. Mr. Charlesworth has not mentioned the great coral-reef situate about three quarters of a mile north of Aldborough, which contains (as far as I have observed) neither univalves nor bivalves, except a few Pectens. I think he must have had this locality in his mind's eye, and thus been led to the misnomer‡.

At the close of the prefatory remarks to my Outline of the Geology of Norfolk, I stated, on the authority of a friend, that there was a bed of shells at Ramsholt analogous to those of the *calcaire grossier* of Paris. I subsequently visited this locality, and found that the bed in question was what might be termed *undisturbed crag*, and the superincumbent bed was

[* Mr. Woodward appears to us to have overlooked the great difference between the organic remains of the red and coralline crag, as shown by Mr. Charlesworth.—EDIT.]

[† We apprehend that this designation as applied to the lower crag by Mr. Charlesworth, refers merely to its being characterized by an abundance of *corals*, having no reference to the bodies termed *corallines*. It is exactly analogous to the appellation "coralline oolite" already established in geology.—EDIT.]

[‡ The coralline crag of Aldborough is expressly referred to by Mr. Charlesworth, p. 86.—EDIT.]

transported, being waterworn and fragmented; this was my impression at the time I visited it, and I see no reason to alter my opinion now; neither do I wish to underrate the merits of Mr. Charlesworth's valuable communication, but speak my sentiments in order that truth may be elicited. The undisturbed bed has a layer of corals of the genus *Astrea*, to which the *Ostrea spectrum* (Leathes) and *Balanus sagittata* are frequently attached; the latter is provisionally named *sagittata* by my friend the Rev. G. R. Leathes, from the shape of the accessory valves, which are frequently found *in situ*. This layer of corals is about three feet below the line of junction with the disturbed crag, and in about the same line I found the tooth of a shark two inches in length, of a light blue colour; and was subsequently presented with two others of the same kind, but smaller, by the gentleman who conducted me to the spot*.

It will be inferred from what I have above stated, that I consider the two beds at Ramsholt to be of the same age; and that the red crag, by diluvial agency, has been superimposed upon the undisturbed bed. There is, however, a bed of crag in Suffolk quite distinct from those at Ramsholt. I allude to that exposed on Thorpe Common, about three miles north of Aldborough; it contains shells specifically like those found in the Norfolk beds, with the vertebræ and tubercles of the thornback and other bones, common in the pits at Thorpe by Norwich. My impression is that the Norfolk crag, extending as far south as Thorpe by Aldborough, is a newer deposit, and that could it be sunk through at the latter place, the regular Suffolk crag shells would be found. One fact in favour of its more recent origin is, that a few of the characteristic shells of the Suffolk crag have been found in it near Norwich, as *Murex contrarius*, *Pectunculus variabilis*, &c., in a bouldered state.

My view of the tertiary deposits of the eastern coast of England is, that in the antediluvian period this island formed a part of the continent, and that the London clay deposit contains the exuviæ of an inland sea which communicated with

* The gentleman here alluded to is Mr. W. S. Fitch of Ipswich, who does honour to science by his zeal in collecting for the pleasure of enriching the cabinets of others. That gentleman presented me with a bouldered tooth of the shark, black and shining, which measures three inches across its base! I think from its worn state that this must equal any of the Maltese teeth. I cannot close this note without bestowing the meed of praise on my friend S. V. Wood, Esq., of Hasketon, near Woodbridge, whose collection has been so highly and deservedly spoken of by Mr. Charlesworth, for the liberality with which he distributes his treasures.

the ocean towards the south;—that at a subsequent period an irruption of the North Sea took place, forming an estuary across Norfolk; its eastern line running from Cromer towards Lowestoft, and from thence across to Belgium, and its western from Weybourn, by Norwich, Bungay, Ipswich, and a part of Essex, thus communicating with the Paris basin. That the Suffolk beds were deposited before the Norfolk, I conclude from the analogy the former bear to those of the Paris deposit, and the latter bearing a greater specific relation to the recent *Testacea**. The fact of the number of mammalian remains found eastward of the estuary thus laid down, even in the bed of the German Ocean, is considered by most as sufficient evidence that it was once dry land; and that its western border was near the line pointed out, may be affirmed by the number of mammalian remains found imbedded with the littoral rejectamenta. The fine grinder of the mastodon figured by Smith in his "Strata Identified," was found at Witlingham: I have a fragment of another which I found at Bramerton; and recently portions of two other grinders have been found in that locality. Of the elephant I have frequently met with fragments of its tusks; but not of its grinders, until this year, when on a visit to the Thorpe pit, I found a large grinder, of thirteen laminæ, thirteen inches long and five across the grinding surface. Some bones have also been found in Bramerton, as an astragalus, vertebra, &c. Bones of the ox, and horns and teeth of the deer have been repeatedly found in this deposit.

I am, Gentlemen, yours, &c.

Norwich, Aug. 15, 1835. SAMUEL WOODWARD.

XLI. *On certain Points in Meteorology and Magnetism. By Sir* G. S. MACKENZIE, *Bart., F.R.SS. L. & E., &c.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

AS there have been marked changes in our climate within a few years, it is desirable that every observation which has apparent importance should be recorded, and the attention of meteorologists called to it. I have made one that

* This opinion was submitted to the Geological Society through Dr. Fitton in 1825, and is noticed in their Proceedings, vol. i. p. 93. [A notice of Mr. Woodward's paper will also be found in Phil. Mag. and Annals, N.S., vol. v. p. 140.—EDIT.]

I consider worth communicating to you. As long as I can remember, the equinoctial gales came from the westward, until the westward variation of the magnet ceased to proceed, and it began to retrograde towards the north, when the gales have come from the eastward. Easterly gales have this year preceded the equinoxes, and at this moment it is blowing hard from the east at the place whence I write. This leads me to think that it would be of importance for those who have the charge of meteorological registers, to examine them in reference to changes in magnetic variation during past times, as far back as registers go; and to note whether any remarkable changes in climate have taken place; and more particularly to ascertain whether the fact respecting the change in the direction of the equinoctial gales has been universal in Great Britain, or confined to the northern parts of the island. Should it be found that there is a connexion between atmospheric phænomena and terrestrial magnetism, and that the movements of the magnetic meridian are accompanied by change in the direction of periodic winds, we may come at length to ascertain some laws by which nature is governed hitherto unknown to us.

I have also observed that the east wind, which has heretofore been remarked as the driest, has of late come to us much charged with moisture.

I beg leave to take this opportunity of remarking, that I conceive no dependence can be placed on the thermometric observations made in the garden of the Horticultural Society. It is some time since I pointed out to Professor Lindley the defects of the apparatus. Instead of the thermometer being placed in the shade of a wall, it is exposed near the ground under a wooden roof which absorbs the direct rays of the sun, and radiates heat to the instruments. Thus the indications are too high.

As it is of importance that the instruments used in meteorological observatories should all agree exactly; and as it is known that even those constructed by the same maker do not always agree with one another, all the instruments in Europe should be compared. Indeed, one maker should be appointed to construct every instrument used in observatories, so that all may be made to agree with a standard.

I am, Gentlemen, yours, &c.

Coul, Sept. 22, 1835. G. S. MACKENZIE.

XLII. *On Divergence as the Cause of Motion in Plants.* By HENRY JOHNSON, *M.D.**

IN a former communication I have given a description of the phænomena of *divergence*, and I have endeavoured to prove the analogy of this property to the irritability of the animal system †. I shall now attempt to show that the cause of divergence is the cause of the motion in plants.

I do not think it necessary to enumerate the many instances and varieties of vital or other motion which are met with in works on vegetable physiology. Instead of extending my remarks to vegetable motion in general, I shall at present confine my observations to one kind, which, from its frequent occurrence and the very obvious nature of its effects, is particularly well suited to experimental illustration.

It seems to be essential to the healthy condition of most plants, that the upper surface of their leaves or their blossoms should be exposed to the light of the sun. I think, indeed, that there are very few, if any, of the more perfect vegetables, to which it is immaterial whether their leaves and flowers be exposed to the light or not. Such plants have, therefore, been endowed with the power of directing these parts to the light, and this motion is effected by a curvature of the stem, leaf-stalk, or flower-stalk. If a young sunflower or hollyhock be laid prostrate, the stem curves, and the apex becoming erect, the leaves are again placed in their usual relation to the rays of light. Where the stem is rigid or fixed, it is the leaf-stalks which perform this motion, as in ivy, of which I have seen the leaf-stalks contorted in the most extraordinary manner from their efforts to bring the glossy surface of the leaves under the influence of the light. I remember to have observed a *Campanula* which had been blown down by the wind, and the stem of which being too rigid and ligneous for it to be capable of curvature, the flower-stalks became curved, so as to erect the fruit, present it to the light, and preserve it from the humidity of the soil.

That motion of vegetables by which they regain the erect posture when laid prostrate, or direct their leaves towards the light, (with the exceptions afterwards to be mentioned,) is always attended with curvature of the stem, leaf-stalk, or flower-stalk; and I proceed to state the experiments and observations which convince me that the property of divergence is the essential cause of this curvature and the consequent motion above described.

* Communicated by the Author.

† See Lond. and Edinb. Phil. Mag., March 1835, vol. vi. p. 164.

I. *Effects of dividing a Stem which has undergone Curvature.*

Exp. 1.—A Jerusalem Artichoke (*Helianthus tuberosus*) was tied down horizontally to a stick. In twenty-four hours the stem had curved, so that its head was directed perpendicularly upwards. The stem was then divided at right angles to the direction of the curvature. The annexed sketch (fig. 1.), a faithful copy of one traced from the plant itself, represents the appearances before and after division. The letters *a, b, c* mark the curved stem previously to division; *d, d* the upper, *e, e* the under or outer, segment after division. The former has become more, and the latter less, curved than the original stem.

Fig. 1.

Helianthus tuberosus.

Exp. 2.—Another stem of the same plant was tied down as in the above experiment. In fifteen hours it had made a neat and obvious curve upwards. It was cut down, and the extremity and curved part were found strongly divergent on division. The upper segment became still more arched than before, whilst the lower lost its curvature and became almost straight.

II. *Effects of removal of upper Segment.*

Exp. 3.—A healthy stem of Spearmint (*Mentha viridis*) growing very erect, was carefully pegged down in a horizontal position. I then cut away the upper side for about the space of one inch and a half. The lower segment immediately hung down almost vertically. At the end of three days this plant died, the mutilated part not having at all curved upwards; but the uninjured part of the stem below had arched irregularly so as partly to erect the stem.

Exp. 4.—Two healthy stems were divided down the middle to the distance of some inches. By means of pegs they were firmly fixed down on the ground, the upper segment having been removed in each. They did not begin to turn upwards until five days after.

III. *Effects of Notching.*

Exp. 5.—Two Jerusalem Artichokes, nearly of the same size, were secured, in the same inclined position, each to a separate stick fixed in the ground. The one was then notched

on the under, and the other on the upper, side. Both immediately became slightly inclined towards the entire side. The effect was still more marked on the next day; and on the third they were both "nodding," as it were, towards the uninjured side.

Exp. 6. (Sept. 15.)—Two stems of the Jerusalem Artichoke were tied down horizontally, and all leaves removed except the terminal bunch.

In fig. 2. several notches were made in the upper side, and it became immediately slightly bent downwards. In fig. 3. the notches were made in the lower side, and it retained its position unchanged. Next day (Sept. 16), fig. 2. was much more curved downwards, and fig. 3. distinctly arched upwards. On the 17th the extremity of fig 3. was again directed upwards. The other was still horizontal.

Sept. 21. That notched on the under side (fig. 3) was curved more than enough to make its head erect. The other, notched on the upper side, (fig. 2.) was now almost upright, but rather less arched than the first.)

Fig. 2.

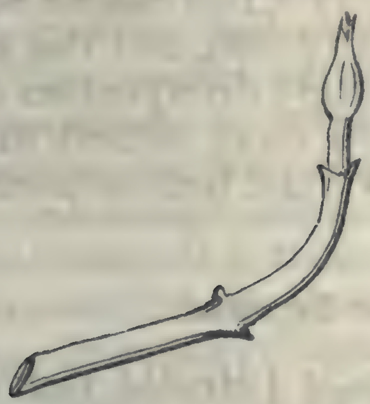
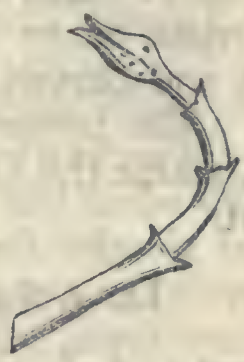


Fig. 3.



Exp. 7.—A very young stem of Jerusalem Artichoke was tied down, and the stem then notched on the upper side. The notches were made near together, and pretty deep, the leaves having been previously removed. The head immediately hung downwards. In twelve days it was cut down. Though the position was somewhat changed, its power of erecting itself seemed to have been destroyed. The notched part still inclined downwards, and not even the apex was turned upwards.

IV. *Parts which are not divergent are not capable of Motion.*

a. The living sprays of all trees, and all herbaceous stems which have become rigid and acquired ligneous matter, are, I believe, invariably incapable of raising themselves up by curving, if laid down, and they are not divergent on division.

b. Many plants which whilst young were capable of motion and divergent on division, lose the property of diver-

gence, and with it the capacity for motion, when they become old, as after flowering.

Exp. 8.—I bent down two spikes of Lavender (*Lavandula Spica*), the one being a very young one not yet in flower, the other almost out of flower. The former raised itself up again, the latter did not. The former was divergent, the latter not.

c. Whilst engaged in the experiments of which I am now giving an account, it was my constant practice to carry a lancet about me, and on every known plant which occurred in my rambles to try the effect of that simple experiment by which it is ascertained whether a plant be divergent or not. The list of plants in which this property was so detected is, as before stated, too long for insertion, but I shall enumerate the principal exceptions, which are not numerous.

1. *Juncus conglomeratus.* Probably every species of Rush.
2. *Iris Pseudacorus*; the leaves divided lengthwise through their narrow diameter.
3. Garden Carnation (*Dianthus*); the flower-stem.
4. Indian Corn (*Zea Mays*?), culms of Wheat, and other Grasses.

Now, it is a most curious fact, that none of the plants or parts of plants above mentioned curve up like ordinary divergent vegetables. It is immaterial to the different species of Rush in which position their green culms be placed; and they are therefore not endowed with the power of raising themselves up when laid down. Besides, from their great flexibility and slender form they are little exposed to this accident, and have plenty of successors or neighbours which, springing up around them, supply their place, if they be destroyed.

2. The leaves of Iris, so far as I have been able to observe, are equally incapable of vital motion as they are of *curving*, and they are destitute of divergent power, except when divided through their long diameter.

3. Every monocotyledonous plant with a jointed stem is either destitute entirely of divergence, or possesses it only in a slight degree; and they are equally without the capacity of curving their stems. Let a culm of one of the *Gramina* be pegged down horizontally on the ground, and it will gradually raise itself up. But this is not done by curving the stem, as in other plants, but the joints assume an angular figure, and thus the stem becomes erect, as seen in the drawings of a gigantic grass, the Indian Corn plant. See figs. 4 and 5. (next page.)

It is perfectly evident from this, that the moving power resides, not in the stem generally, but in the neighbourhood of the joints. And as the intermediate space (except when very young) is not divergent on division, we have here, in my

opinion, a most curious and unexpected illustration of the theory which I have been attempting to prove.

Fig. 4.

Let us now review the facts and experiments which have been adduced, and see what may be reasonably made out from them.

It has been proved by dividing the stem of a plant which has already undergone curvature upwards, (Exp. 1. and 2.) that both the segments are endowed with the property of divergence.

It is evident also from the same experiments, that the lower segment cannot contribute to this motion, as any effect which it may have must be greatest in an opposite direction.

The motion or curvature upwards must therefore depend on the other, the upper segment; and when we see the great, the excessive force with which the upper segment retracts on being freed from the antagonism of the lower segment, we can hardly fail to believe, that in this retraction (or rather in its cause) we have discovered an adequate, and I think the true source of the motion.

Fig. 5.

Hence, I come to the conclusion, that the kind of motion here treated of is the result of the contractile force or divergence of the upper segment prevailing over that of the lower.

It will, no doubt, immediately occur to the reader, that to substantiate the above inference it is incumbent on me to show, that removal or destruction of the upper segment is followed by loss or diminution of the power of curving upwards. Now although, on account of the facility with which new matter (probably endowed with moving fibres) is added to mutilated plants, it be very difficult entirely to prevent stems curving upwards by removal of the upper segment, yet I have occasionally seen the experiment succeed, and I can confidently affirm, that the process, if not prevented, is much retarded, and of this fact the experiments above described are a sufficient proof. In Exp. 3. the part of the stem of Spearmint from which the upper segment had been removed had not curved upwards at the end of three days, and in Exp. 4. this action did not begin until five days after. In the natural state this would certainly have occurred in about twenty-four hours.

We conclude, then, that as in the first series of experiments the cause of motion was traced into the upper segment of a divided stem, so we here find that removal of this part *prevents* or *retards* the same motion.

The experiments on the effects of notching curiously confirm and illustrate the inferences drawn from the preceding experiments.

By the process of notching, we artificially weaken the contractility of one side, and enable that of the other to exert itself so as to arch the stem. In this way curvature or motion in almost any direction may be produced at will, as in Exp. 5. Exp. 6. shows the effect of notching in delaying the motion upwards; one stem notched on the upper side was still horizontal on the second day after the operation.

In Exp. 7. the process of notching has so completely destroyed the action of the upper portion of the stem, that at the end of twelve days the stem was still inclined downwards, and even the apex was not turned upwards.

If anything more be necessary to establish the fact that divergence is the cause of the motion in plants here treated of, it is to show that the want of this property is attended with the want of the power of motion. And this I think is clearly made out by the facts stated in section IV.

I have thus endeavoured to trace a certain kind of vegetable motion to that property which I have termed divergence, the analogy of which to irritability or contractility I have laboured to prove, 1st, by its supposed vital nature; 2ndly, by its being sensible to stimulants; and 3rdly, by its being the cause of vital motion.

Shrewsbury, July 18, 1835. HENRY JOHNSON.

XLIII. *On certain coloured Bands observed in examining Newton's Rings.* By A. R.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In examining Newton's rings I was induced to place a convex lens (36-inch focus) between two surfaces of plate-glass, in order to effect the superposition of the rings.

My attention was suddenly diverted when viewing the rings with transmitted light, by observing a series of coloured bands extending nearly over the whole surface of the lens, but not assuming any definite form, nor did the breadth or direction of any of these bands bear the slightest relation to the situation of the rings; their curvature varied materially with a variation in the pressure; and their breadth changed from the same cause. Having satisfied myself that these appearances were not owing to any films, or particles of dust, I endeavoured to obtain them by reflection, and succeeded equally well. In examining them with glass of different thickness, the magnitude and curvature of the bands differed materially for the *same degree* of pressure, and it appeared that for extent of surface and brilliancy of colour, two pieces of plate glass of equal thickness are the best. These effects were observed by the light of a candle, and on the following day I found that they were not produced by the sunlight until received into a darkened room through a very small orifice, when the maximum effect was produced by placing the lens at its focal distance from the orifice. The breadth of the bands is greatest for the same pressure when the incident beam of light is $\angle^{r}$ to the surface of the glass.

Polarized light produced no change, except when the analysing eye-piece of Iceland spar was in such a position as not to transmit the reflected beam, in which case the darkness was too intense to render the colours visible. I have repeated this experiment with glass varying in thickness from the twelfth of an inch to three fourths of an inch; in the latter the bands could scarcely be discovered, and were then visible only in the immediate neighbourhood of the rings.

These are the principal facts I have observed, and their explanation will probably be found to depend upon the interference of the waves of light reflected and transmitted at the different surfaces of the glass, and this is partly confirmed from my being unable to obtain the bands when a lens of short focal length is used. Whether this experiment will aid in establishing the greater certainty of any branch of the undu-

latory theory of light, time alone can determine, and some relation must be established between the compressing force and the breadth and forms of the rings before we should be warranted in deducing any conclusions.

I am, Gentlemen, yours, &c.

Liverpool, Sept. 12, 1835. A. R.

XLIV. *On the Verification of* Captain Lloyd's *Levelling Instrument by the Greenwich Mural Circle. By* John Nixon, *Esq.**

IN order to prove that the gun-metal collars of the superb level, by Cary, employed by Captain Lloyd in levelling from London to Sheerness†, had been constructed of equal diameter, the instrument was set up in the north window of the Observatory at Greenwich, at a distance of about eight feet from the mural circle‡. The cross wires of the (thirty-inch) telescope of the level being adjusted to the sidereal focus, by observing when those of the great telescope of the circle, as seen against a disc of white paper placed about an inch from its eye end, appeared perfectly distinct, the horizontal wire of the level was adjusted for collimation, and the great telescope fixed at the (mean) horizontal point of the circle, (which point bisects the arc passed over by the telescope in observing the direct and reflected altitude of a star). On looking through the telescope of the level, its horizontal wire was found to conceal or cover that of the circle so completely that the wires could not be seen separate without a slight elevation of the telescope of the level. In confirmation of the correctness of the observation, the great telescope, previously inclined, was moved until its horizontal wire coincided precisely with that of the undisturbed level. On reading off the microscopes of the circle, the direction of its telescope proved to be horizontal within a small fraction of a second.

The success of the method must evidently be in proportion to the degree of fineness of the thicker of the two horizontal wires made use of; because, as both may be considered opake, it would be impossible to distinguish with what part of the diameter of the thicker wire the finer one coincided, or, indeed, whether they were merely sufficiently approximated not to appear separate. The wires of the Greenwich

* Communicated by the Author.

† See the Philosophical Transactions for 1831. [or Phil. Mag. and Annals, N.S., vol. ix. p. 368.—Edit.]

‡ Is the measurement from the object-glasses or from the centres of the two instruments?

telescope may be regarded as unexceptionable, but were those of the level equally delicate they could scarcely have been efficient in staff-levelling at short distances.

On describing one of the numerous methods made use of to determine the cylindrical error of my horizon sector, I remarked that the results, which varied from 17″·5 to 27″·5*, were scarcely worth transcribing, owing to the almost impossibility of placing one horizontal wire exactly before another. Unable to comprehend how the method which had been considered successful at Greenwich should fail so signally in the above experiments, it occurred to me that the subject might be best investigated by observing through what (minute) range of inclination of the telescope its horizontal wire could be deemed *passably* coincident with that of the (fixed) collimator. My horizon sector, of which the wires are extremely fine, served as collimator, the light being derived, as in the Greenwich observations, from a piece of white paper set up at the most favourable distance from the eye-tube. The telescope employed (taken from my repeating circle) is fitted up with wires (by Dollond), apparently of the same fineness as those of the sector, placed between the two nearest lenses of the eye-tube. It was mounted with a very large spirit level, of which the divisions of the scale, about thirty-two to an inch, were equal to 1″ each. Both instruments stood on a plank resting on brackets driven into solid masonry; and, as an additional precaution, one of the great levels of the sector served to detect any slight change of inclination in the plank. During the experiments, of which the results are subjoined, both horizontal wires, as the vertical ones were kept coincident, must have been parallel throughout their length.

No. I.—February 7, 1835. Temp. 49° Fahr. throughout.

Point of Scale opposite middle of Bubble.	Appearance of Wires.
°	
115·5	Slightly separate.
117·5	As one wire, but rather thicker.
120·	More like one wire.
121·5	Blacker, and as one wire.
124·5	As one, but rather thicker.
126·	Almost as one wire.
127·5	Penumbra below.
129·5	Scarcely passable.
132·0	A light line below.

* See Phil. Mag. and Annals for 1831, N.S. vol. x. p. 347.

No. II.—Same day. Temp. 49°.

113° —Pale penumbra above.
115 —Very little penumbra.
117 —As one wire, but blacker and thicker.
120 —As one black wire.
122·5—As one thin wire. (Best coincidence.)
125 —Very faint penumbra.
127·5—Penumbra, but might pass.
132· —Not quite separate, but would not pass.
135 —Distinctly separate.

No. III.—February 9, 1835. Temp. 49°. Inclination varied.

124° —Faintly separate.
126 —Penumbra above.
128 —Pale ditto.
130 —Black, and rather thicker than one wire.
132 —Ditto.
135 —As one thin wire. (Best coincidence.)
138 —Black, as one wire, but rather thicker.
139·5—Faint penumbra.
142·5—Just separate.

From the above experiments it would appear that the wires might be considered coincident through a variation of inclination equal to 7″ or 8″.

September 30, 1835. John Nixon.

XLV. *On the Enharmonic Organ; in Reply to an Article in the Westminster Review.* *By Mr.* W. S. B. Woolhouse.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the Westminster Review just published an article appears in reference to my essay on Musical Intervals, &c., in which the writer has thought proper to pass off some very liberal expressions in condemnation of my notice of the enharmonic organ lately constructed by Messrs. Robson and Son. As the motive of the reviewer—who, it will appear, has not so much as digested even a moderate portion of the essay,—seems to have been merely the opportunity of giving supremacy to his *own* ideas on the construction and capabilities of the organ in question, and as I conceive he has, in no small degree, misrepresented my observations, incidentally made, on that subject, I hope you will so far support the fairness and freedom of reply as to give a place to the following remarks in your valuable Magazine.

The principal objection is opposed to my statement, that in

the formation of the instrument the scale adopted is the division of the octave into 53 divisions, instead of which I should have said the true or mathematical division. So far I admit the writer may very probably be correct, as the circumstance depends altogether on the mode in which it has been tuned, and not on its mechanical construction. But when, after stating himself more than once that the difference between the true or mathematical division and that which divides the octave into 53 equal parts is so minute as to be quite inappreciable, he comes forward, and, in allusion to my having taken one of these modes of division instead of the other, to which it so remarkably assimilates, he, without considering for a moment the possibility of any obscurity attaching to the description given in the review, launches out into such expressions as, "There was but a right and a wrong, and the latter has by inadvertency been pounced upon by the author of the essay;" "The author of the essay has run carelessly over the account, and gone away with exactly the wrong idea of it;" "This is what the French would call a *major* oversight;" &c. &c.,—and when in the present article he at one place observes "that if an instrument is constructed (like the one in question) with the sounds of the perfect or mathematical scale, each of these sounds *approaches very nearly* to one of the sounds in the equable division into 53," and in the very same article, to give a forcible impression to his objection, he flatly states that the instrument "contained *nothing like* a division of the octave into 53 equal intervals,"—really his candour becomes very questionable, and we are led inevitably to the conclusion that he is influenced by a desire to find fault rather than to correct, and more disposed to carp at this merely incidental and comparatively frivolous notice of the organ than to exercise his genius in discussing the merits of the work itself.

He proceeds to state that my account of the instrument is "exceedingly inaccurate". Now I completely deny that there is the slightest inaccuracy in my statement concerning its complexity and number of sounds; and I must here again observe, that the reviewer has misrepresented the case. It may be proper to observe, that his extract from my work is not faithfully given. Instead of my saying of the instrument that it would be too much to expect from it complete success, as the superabundance of keys must prove an insuperable objection to the execution," he should have transcribed the last words, "impediment to the execution." This, however, though objectionable as an extract, is comparatively of but little importance. By way of substantiating his charge of inaccuracy, he says, "The fact is, that the division which gives the ma-

thematical or accurate sounds, presents only 16 ordinary sounds or finger-keys in the octave on any one finger-board." But it should be observed that these 16 finger-keys form only a *part* of the general scale of 29 sounds or finger-keys, and are insufficient for the completion of the limited number of keys, viz. from five sharps to four flats inclusive, to which the capabilities of the instrument are said to extend. It cannot be supposed by any sensible person that the "ordinary" finger-keys constitute the impediment to which I allude in my essay, and it must certainly be conceded that the 13 extra additional finger-keys, which make up the 29 sounds, must increase very materially the difficulties of the execution in the cases for which they are required, *i. e.* in the cases in which the instrument asserts its superiority over those of ordinary construction. The writer cannot deny the truth of my statement that the organ contained 29 sounds in the octave, as enumerated in the paragraph he has extracted. I challenge him to do so; and until he ventures to do this, where, Gentlemen, is the ground of his charge of inaccuracy?

Before my notice was written I inspected the instrument, and I here beg leave to express my sense of the very obliging manner in which it was shown to me by a gentleman whom I presumed to be Mr. Robson jun. He told me, as a curious fact, that several professors had practised the instrument without success, and that the only person capable of playing on it at all was blind, and that this person was Mr. Purkis, the celebrated performer on the Apollonicon. After I had seen the instrument, I was favoured with one or two pamphlets giving a description of the organ, and when I came afterwards to peruse them, I was rather surprised to find them exactly the same as the notice given by way of an article in the Westminster Review for January last. This circumstance favours the belief that some connexion exists between the proprietors of the organ and the writer in the Review, and may, perhaps, account for his evident chagrin at my observations and the zeal with which he applies himself to the defence of the character of the instrument and the principles of its construction. I should, indeed, be glad to see it succeed, but at the same time I do think that I ought to be allowed the right of giving a deliberate and candid opinion on the subject, without rendering myself the object of an attack of such a nature as that which is contained in the present number of the Westminster Review. Yours, &c.

August 19, 1835. W. S. B. Woolhouse.

XLVI. *On a New Rotative Steam-Engine.* *By* John Taylor, *Esq., F.R.S., Treas. G.S., &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

It is very well known to those who have observed the duty of steam-engines employed in the mines of Cornwall, that an enormous difference has existed between those which raise water by a reciprocating motion, and those which for other purposes have that motion converted into a rotative one by the intervention of a crank. The cause of this difference has often been speculated upon, but has not, I believe, been well explained: it is important in an œconomical point of view, as while in the pumping engines 60 millions pounds are commonly raised one foot by each bushel of coal consumed, the rotative engines for stamping ores have seldom raised more than 20 millions; and those for winding up the ores from underground are found to be even far below this in effect.

Now, it should be observed that the pumping-engines are at present universally, I believe, single engines, that is to say, receiving the steam from the boiler on one side of the piston only, the principle of working double, as it is called, which was introduced by Mr. Watt, having been for some time discarded; and in these single engines the method of working high pressure steam expansively, which we owe to Mr. Woolf, has long been used with the greatest advantage.

The rotative engines in Cornwall, like all others which are used for manufacturing purposes, are double engines, and receive the steam alternately above and below the piston; and though attempts have been made to work them expansively, these attempts have not been very successful.

The object of my present address to you is to notice an engine which has lately been constructed for a mine in which I am interested, which is a rotative one for stamping tin ores, and which when I visited the mine a few days since was calculated to be performing a duty of about 60 millions, or nearly equal to the average of the better class of reciprocating engines, and nearly three times as much as the best rotative engines have hitherto done.

I wish to call the attention of persons concerned in the use of steam-engines to this fact, because if it should be found that this rate of duty can be maintained, a very great improvement may take place in all such as are most generally employed.

This engine is at work at the Charles Town United mines near St. Austell; it was erected for us under the direction of Mr. Sims, an engineer of great experience in Cornwall. It differs from the general construction in being a single engine, having the beam loaded at the outer end; and the rotatory motion of the crank is rendered almost completely uniform by the assistance of the fly-wheels. It works nearly as expansively as the pumping-engines.

It was predicted, I understand, before the engine went to work, that a steady rotative motion could not be produced in this way, and some believed that the crank would never pass the centre; I can, however, bear witness that the action is extremely good, and will, I believe, by a little alteration in the weight and diameter of the fly-wheels, be made perfect; and as it must be an object to save at least one half the fuel ordinarily consumed, I point it out as deserving attention and inquiry. I have desired that its performance may be regularly reported in the monthly duty papers.

I am informed by Captain Thomas Lean, who reports the duty of most of the engines in Cornwall, that this is not the first construction of the kind, but that a similar one was erected formerly at Wheal Vor tin-mine by Mr. Peter Godfrey, and that it then surpassed in duty any other stamping engine of its day, but that for some reason it never attracted much notice.

Mr. Sims is constructing a winding-engine for the same mine on a similar principle. Yours very truly,

Bedford Row, Oct. 12, 1835. JOHN TAYLOR.

XLVII. *A Sketch of the Geology of West Norfolk.* By C. B. ROSE, *Fellow of the Royal Medical and Chirurgical Society of London.*

[Continued from p. 279.]

Chalk with Flints.—MEDIAL chalk of "Geology of Norfolk."

The escarpment of this bed of chalk (incumbent on the last) forms the high range or downs of Norfolk, running nearly due north and south from Thornham and Brancaster through Docking, Great Massingham, Lexham, Litcham, Castleacre, Swaffham, Hilborough, Saham-Tony, Weeting, to Thetford. From this range flow the principal rivers of the county; two only, the Setchy or Nar and the Wissey or Stoke, cutting through it in their western course to the sea.

This bed is well characterized by its numerous horizontal

strata of tabular flints, placed at distances from each other, varying from three to eight feet. At Swaffham two layers occur but twenty inches asunder*; here also the tabular flints are of great magnitude, many of them being eight or more feet in length, and from nine to twelve inches in thickness. Many nodules of flint are interspersed throughout the chalk, the majority of them small, and receiving their form from a zoophitic nucleus; larger flints thus situated are less frequent: the *Paramoudra* is very rare, I have seen but one; but at Thornham the chalk "contains enormous Paramoudræ†." At Hilborough the uppermost layer of flint, at the depth of twelve feet from the surface, is a thin seam, varying in thickness from one eighth to an inch, some of the thicker parts having the appearance of two thin plates cemented together; about eighteen inches below this occurs a layer of nodular and cylindrical flints, running horizontally, and parallel to the seam. Similar thin seams were observed near Wells by Mr. Woodward, "traversing the chalk, both in a horizontal and oblique direction; and in many parts they do not exceed one eighth of an inch in thickness‡." I found also at Thetford thin tabular flint, not more than half an inch in thickness; the first layer occurs at about ten or twelve feet below the surface, and lies parallel to the stratification of the chalk: the lime-burner informed me that at the depth of twenty feet three layers of similar flint succeed each other at about a yard asunder, and that the flint is invariably found fractured in every direction. Each slab of flint appeared to be formed of two plates in apposition, the flint in the line of junction being of a lighter shade: this character was very apparent upon the recently fractured surfaces. In one instance I observed drusy crystals of quartz between the plates. Does not this laminated structure offer an illustration of the mode of formation and arrangement of the flinty strata? and does it not favour the opinion, that the siliceous molecules, by elective attraction, separate themselves from the calcareous matter, above and below certain parallels determined by the proportion of silex contained in the chalk, and approach each other until they arrange themselves in the tabular form §?

The chalk at Thetford abounds in flints, which partake

* Mr. Woodward informs us that similar "double rows of flints" occur in East Norfolk. See Geology of Norfolk, p. 24.

† Mr. R. C. Taylor's Paper in the Geological Transactions, Sec. Ser., vol. i. p. 378.

‡ Geology of Norfolk, p. 27.

§ When the masses of clay mixed with ground flints, prepared for making fine pottery and china, "are allowed to stand unused for some time, it often happens that the particles of the powdered flint separate

more of the cylindrical form than the nodular; many of them are from eighteen to twenty-four inches in length, and are covered with numerous irregularly formed asperities: these flints are scattered through the chalk like the nodules at other localities.

At Litcham the layers of flint are placed from four to six feet asunder; their horizontal course is not a plane, but slightly undulating: at the depth of twenty feet a double row occurs, the two layers being about nine inches asunder.

Our flint is of various shades, from black to gray, and has frequently, in addition to the white coating and lighter zones common to flints, small white circles upon their outer surface, the presence of which I cannot account for. Many flints exhibit elegant arborizations on their fractured surface, the stone separating more readily where these vegetations (?) occur; similar dendrites are seen on chalk, and always between the natural divisions of the mineral: these beautiful dendritic configurations possess somewhat a metallic lustre upon the black flint when first exposed, and are black upon the gray flint, and soon lose their lustre; they frequently rival the happiest efforts of the artist's pencil.

The texture of this bed of chalk has an intermediate degree of hardness to that of the lower beds, and those situated to the *east* at Norwich, belonging to higher strata. Although there is a progressive increase in the compactness of the chalk downwards, still the hardness varies without any order: blocks, and even fragments of blocks, possessing a hardness not inferior to the lower chalk, are met with in all parts of the bed, but more particularly in the immediate vicinity of the layers of flint; still the chalk in the latter situation is not invariably the hardest.

The fissures, or "pipes", occasionally seen in the chalk of Sussex, and also in the "upper" chalk at Norwich*, are rarely observed in the "medial" and lower beds. The largest I have seen in this neighbourhood were in Mr. Lynes's pit at Litcham; they were lined with a brown adhesive clay, and their centre filled with gravel; they are now obliterated. Smaller pipes occur in a pit at Swaffham, and upon examining the inner surface of the chalk, I found it vertically striated, proving that they were filled from the upper surface: the clay

from the clay into detached hard stony nodules. The observation of this fact has thrown considerable light on the probable origin of the nodules of flint in chalk, a subject which was very obscure, and of which no satisfactory theory had previously been proposed."—Article Iron, No. 2; in No. 161. of the Penny Magazine, October 4, 1834.

* Geology of Norfolk, p. 27.

and gravel were arranged in the same manner as in those at Litcham.

Mineral Contents.—Drusy crystals of quartz, with botryoidal and mammillated chalcedony, both found lining the cavities of flints, and pyramidal and rhombic crystals of carbonate of lime, occupying the interior of shells and echini, are the only minerals we possess: the latter are found in cavities in the chalk in Sussex; they have not at present been discovered in those situations in Norfolk, that I am aware of. Calcareous tufa is deposited by the springs at Swaffham; the interior of one of the wells was found encrusted to the depth of thirty feet: it commenced about twenty feet from the surface, and portions of it were two inches in thickness; the texture of some of it was porous, other portions were compact, and the surface generally subcrystalline.

Sulphuret of iron is abundant; it occurs for the most part in spherical balls, having a fibrous structure, radiating from the centre, and terminating on the surface in tetrahedral pyramids: they frequently inclose a Terebratula as their nucleus. Clusters of columnar crystals of this mineral, as figured by Mantell in "Fossils of the South Downs," tab. 16. fig. 11. are found in the chalk at Swaffham.

The black oxide of manganese is met with at Castleacre. I found this black power lying in the natural separations of the chalk, about twenty feet from the surface; it occupies both the oblique and horizontal clefts, but is most abundant in the latter; it is accompanied by and partly mixed with brown oxide of iron and loose chalk. The thickness of this bed at Diss is 330 feet: Hunstanton lies to the west of its escarpment, so that it does not extend to the cliff.

The *organic remains* are numerous, and the living animals appear to have had favourite localities in the chalk ocean, some individuals abounding in one spot, and not appearing at another: thus, *Inoceramus Brongniarti* and *I. striatus* abound at Westacre; *I. intermedius* at Narborough; *I. cordiformis* at Swaffham; *Pecten Beaveri* at Sandringham and Hunstanton, rare at Marham; *Plagiostoma spinosa* at Thetford; *P. Hoperi* at Swaffham; *Inoceramus involutus* at Saham, very rare at Swaffham; *Echinus saxatilis* at Litcham; *Cidaris cretosa* at Swaffham. Others, inhabitants of the early period of the chalk formation, have vanished in the later period; among these are the *Ammonites* of the lower beds, *Pecten Beaveri*, *Spatangus hemisphæricus*, *Galerites Hawkinsii*, &c. These have been replaced by *Ananchytes*, other *Spatangi*, and large *Belemnites*; the latter are rare, even in the "*medial*" chalk, for but one has been found at Swaffham. I have also remarked of

these *Echinites* and *Testacea*, which are distributed through *all* the beds of the chalk, a very large proportion are the exuviæ of young animals: full-grown animals are, in the *lower* beds, comparatively rare; I may particularize the *Gryphæa globosa*, *Ostrea canaliculata*, also the oyster so commonly attached to *Echini*, and *Ananchytes hemisphæricus*: thus in the adult shells of the *upper* beds we possess "natural chronometers," affording a measure of the period during which the chalk deposition was in progress.

Name.	*Reference.*	*Locality.*
AGAMIA.		
Confervites*	undetermined.	Swaffham.
POLYPI.		
Flustra utricularis	Geol. Norf., t. iv. f. 7.	Ditto, Litcham, &c.
——— retiformis	Ibid. t. iv. f. 6.	Ditto.
Lunulites urceolatus	Ibid. t. iv. f. 9.	Ditto, ditto.
Millepora truncata†	Ibid. t. iv. f. 13.	Litcham.
——— polymorpha	Ibid. t. iv. f. 13.	Ditto.
——— globularis	Geol. York., p. 1. f. 12.	Ditto.
Caryophyllia centralis	Geol. Suss., t. 16. f. 2, 4.	Swaffham, Litcham, &c.
Madrepora	undetermined.	Ditto.
Spongia bifida	MS. catalogue.	Ditto.
———‡	....	Ditto, Litcham.
——— Townsendi	Geol. Suss., t. 15. f. 9.	Ditto.
Polypothecia palmata	MissBenett'sCat., t.11.f.1.	Ditto.
——— clavellata	Ibid. t. 12.	Saham.
Paramoudra	Geol. Trans., vol. iv. t. 24.	Swaffham, Thornham.
Choanites subrotundus	Geol. Suss., t. 15. f. 2.	Ditto.
Ventriculites radiatus§	Ibid. t. 10. f. 13.	Do. Saham, Thetford, Litcham.
——— Benettiæ?	Ibid. t. 15. f. 3.	Swaffham.
RADIARIA.		
Asterias semilunatus	detached ossiculæ.	Ditto, Litcham.
Apiocrinites ellipticus‖	Miller, Hist. Crin., p. 34.	Saham, Ditto.
Cidaris cretosa	Park. Org. Rem., vol. iii. pl. 4. f. 3.	Swaffham, Litcham.
Echinus saxatilis	Ibid. pl. 3. f. 1.	Litcham.
——— Königi	Ibid. pl. 1. f. 10.	Thetford.
Spines, acicular¶	cucumerine & clavated.	Swaffham, Litcham.
Galerites albogalerus	Geol. Suss., t. 17. f. 8.	Ditto, ditto.
Ananchytes scutatus	Org. Rem., vol iii. pl. 2. f. 4.	Ditto, ditto.
——— hemisphæricus**	Smith's Chalk Plate, f. 10.	Ditto, ditto. [&c.
Spatangus cordiformis	Geol. Norf., t. 5. f. 6.	Do., Thetford, Saham,
——— rostratus	Geol. Suss., t. 17. f. 10, 17.	Do., Litcham.
——— excentricus††	Geol. Norf., t. 1. f. 5.	Ditto.

* On the chalk and flints.
† Two or three species or varieties, undetermined.
‡ Tubular, allied to Siphonia.
§ Vide my paper in Magazine of Natural History, vol. ii. p. 332.
‖ Bottle Encrinite; Park. Orcyt. pl. 9. f. 13.
¶ Measuring 4½ inches long. ** Young of. †† Unique.

Names.	*References.*	*Localities.*
ANNELIDES.		
Serpula spirulæa.........	Sow. Gen. Shells, No. 22.	Swaffham, Litcham.
——— ampullacea ...	Min. Con., t. 597. f. 1--5.	Ditto, ditto.
——— granulata	Ibid. f. 7, 8.	Ditto, ditto.
——— fluctuata.........	Ibid. t. 608. f. 5.	Ditto, ditto.
——— plexus............	Ibid. t. 598. f. 1.	Do., do., Thetford.
——— obtusa	Ibid. t. 608. f. 8.	Saham.
CIRRIPEDA.		
Pollicipes maximus......	Ibid. t. 605. f. 3–6.	Swaffham.
CONCHIFERA.		
Inoceramus Cuvieri......	Ibid. t. 441. f. 1.	Do., Litcham, Thetford.
——— Brongniarti ...	Ibid. f. 2.	Do., do., Hilborough.
——— cordiformis....	Ibid. t. 440.	Ditto.
——— involutus	Ibid. t. 583.	Do., Lexham, Litcham, Saham.
——— latus	Ibid. t. 582. f. 1.	Ditto.
——— striatus	Ibid. f. 2.	Ditto.
Dianchora lata*	Ibid. t. 80.	Do., Litcham, Saham, Thetford.
Plagiostoma spinosa ...	Ibid. t. 78.	Ditto, Thetford.
——— Hoperi	Ibid. t. 380.	Ditto, Litcham.
——— granulosa	Geol. Norf., t. 5. f. 26.	Ditto
Pecten nitidus	Min. Con., t. 394. f. 1.	Do., Gooderstone, Saham, Litcham, Thetford.
——— concentricus ...	Geol. Norf., t. 5. f. 27.	Saham.
Gryphæa globosa jun....	Min. Con., t. 392.	Swaffham.
Ostrea alæformis jun....	Geol. Norf., t. 6. f. 1.	Ditto, Litcham.
——— canaliculata jun.	Min. Con., t. 135.	Ditto.
Crania Parisiensis	Ibid. t. 408.	Ditto, Litcham.
——— striata............	Geol. Norf., t. 6. f. 15.	Ditto, Saham.
Terebratula striatula ...	Min. Con., t. 536. f. 4.	Ditto.
——— Mantelliana ...	Ibid. t. 537. f. 5.	Ditto.
——— plicatilis.........	Ibid. t. 118. f. 1.	Ditto.
——— obliqua	Ibid. t. 277. f. 2.	Ditto.
——— subplicata	Geol. Suss., t. 26. f. 5.	Ditto, Castleacre.
——— subrotunda......	Min. Con., t. 15. f. 1. 2.	Ditto, Litcham.
——— semiglobosa ...	Ibid. t. 15. f. 9.	Ditto, do., Thetford.
Magas truncata	Geol. Norf., t. 6. f. 9.	Ditt o.
MOLLUSCA.		
Cirrus perspectivus......	Min. Con., t. 428.	Ditto, Litcham.
Turritella multicostata	MS. catalogue.	Ditto.
Belemnites mucronatus	Min. Con., t. 600. f. 2.	Litcham.
——— granulatus......	Ibid. t. 600. f. 5.	Ditto.
Nautilus elegans jun. ...	Ibid. t. 116.	Swaffham, Lexham.
Ammonites, a fragment,	species undetermined.	Litcham.
Scaphites obliquus	Min. Con., t. 18. f. 4. 5.	Swaffham.

* Both valves found at Swaffham. The Marsupite has been found at Swanton Morley.

Names.	*References.*	*Localities.*
PISCES.		
Diodon, very gibbous and sulcated palatal teeth, from Saham.		
Palatal teeth resembling those of *Squalus Perlon**, from Swaffham.		
Squalus Zygæna, teeth	Geol. S.E. England, p. 132. f. 5.	Swaffham, Litcham, Docking, and Thetford.
——— Mustelus, teeth	Ibid. figs. 3. 4.	Swaffham, Thetford.
——— galeus†, teeth	Geol. Suss., t.32.f.12,14.	Ditto, Saham.
——— Phillipsii‡, teeth	Ibid. t. 32. f. 22.	Ditto.
——— a vertebra	....	Do., Litcham, Lexham.
Zeus Lewesiensis §	Ibid. t. 35. f. 1.	Ditto.
Esox Lewesiensis.........	Ibid. t. 41. f. 1.	Ditto, Saham.
Scales, very imperfect...	....	Ditto, ditto.

The usual series of *tertiary* beds overlying the chalk is not met with in Western Norfolk.

[To be continued.]

XLVIII. *An Inquiry into the Nature of the Structure of Rocks.* *By* HENRY S. BOASE, *M.D., &c.*¶

IN the Transactions of the Geological Society, (Second Series, vol. iii. part 3.) just published, there is a paper from the pen of Professor Sedgwick, "On the Structure of large Mineral Masses**," in which opinions are advanced very different from those advocated in my "Treatise on Primary Geology."

I have therefore been induced to enter once more on this subject, in the hope of originating a discussion which may ultimately tend to reconcile, if possible, those points on which we disagree. And it fortunately happens, that of all geological speculations there is none, perhaps, more tangible than that concerning the nature of the structure of rocks; none which can be more readily submitted to the test of facts; and none, therefore, from which we may expect to derive more satisfactory results.

The Professor, in the very first sentence of his paper, states that "all *solid* mineral masses *must* have undergone some change since the time of their first production." If it be intended only to imply by this, that all mineral masses are not now in the same state as when their incoherent materials were originally deposited, then every one must admit the correctness of this assertion; but if, on the other hand, this state-

* Geol. S.E. of England, p. 132. fig. 7.
† Resembling those of *Sq. Cuvieri.*
‡ Species unknown, Geol. S.E. England, p. 132, f. 6.
§ Part of a spine; the vertebræ very tender.
¶ Read before the Royal Geological Society of Cornwall; and communicated by the Author.
** [An Abstract of Prof. Sedgwick's paper here referred to, was given in our last number, p. 320.—EDIT.]

ment be taken in its literal meaning, I am not prepared to concede that all mineral masses have experienced *structural* changes since the period of their consolidation. The point at issue between us, in this instance, may be thus stated: the Professor supposes that rocks have acquired some kinds of structure by the application of mechanical force—sometimes external—since they have been in a solid state; whereas, on the other view of the subject, all kinds of structure are accomplished when rocks become perfectly solidified.

A detailed consideration of the subject will, however, more clearly show the merits of either opinion; and, in the first place, the structure of the granitic, trappean, and other igneous rocks will engage our attention.

"No one supposes," observes the Professor at page 461, *ubi sup.* "that columnar basalt was originally built up of solid parallel jointed pillars, or that the structure of a granitoid rock was effected by a mere fortuitous concourse of the crystalline parts. We believe that these phænomena are the necessary consequences of a certain anterior condition of the materials. .... The mass which has changed its temperature, and become solid, has also changed its dimensions. Contraction must produce tension on the whole mass; and this tension, acting mechanically, will in many instances produce joints and fissures, and sometimes contortions." On this it may be remarked, that the peculiar geometrical forms of these rocks have certainly not been produced by a mechanical nor by a fortuitous arrangement; but have probably, at the time of consolidation, resulted from a process very similar to, if not identical with that of crystallization: and further, it may also be maintained, that the joints are the bounding planes of such crystals, having, therefore, not a posterior, but a synchronous origin with that of the solid concretions. Now, these joints may be more or less distinctly defined, as in the crystals of various simple minerals; and may have been rendered more strongly marked by open spaces and other alterations, subsequently effected by the operation of mechanical and chemical causes; but the occurrence of such fissures does not necessarily imply that the joints of rocks have been occasioned by a tension arising from contraction of the mass during cooling. Indeed, we know little experimentally concerning the contraction of rocks in passing from the fluid to the solid state; and it is even possible that some of them, like other bodies, may actually expand during such a change of condition. Even admitting that contraction does take place, we have yet to learn that it would operate on innumerable small portions,

instead of equably affecting the whole mass, especially whilst subjected to immense pressure. But we know that such fissures, at or near the surface of the earth, can be, and in numerous cases probably have been, developed by the percolation of water and other atmospheric causes, acting in the direction of previously existing joints: thus unfolding the forms of the rock-concretions, just as the fundamental figures of a crystalline mass are displayed by the partial action of a chemical solvent; an analogy confirmed by the fact, that large blocks of granite, or the mass itself in which the joints are not visible, may be mechanically divided into quadrangular portions, corresponding in form to those produced by the action of the elements.

This subject will be more particularly enlarged on hereafter, when the joints of sedimentary rocks come under consideration. But it may be proper to offer now a brief sketch of the structure of igneous rocks, in order to compare it with that of other formations, and to test the opinion which I have elsewhere advanced, "that the structure of rocks is not dependent on any one particular mode of formation, for each individually, whether igneous or aqueous, by placing the mobile particles under circumstances favourable to the exertion of cohesion, effects the same object by different means."

When igneous rocks occur in large masses, they are found to be traversed by systems of parallel joints or fissures, by which the whole mass is divided into blocks, varying in size and figure according to the nature of the rock. Sometimes the forms of the resulting blocks are very irregular; but at other times they possess such a surprising symmetry of dimensions that they deserve to be termed crystals. A closer inspection shows that the minerals of which these blocks are composed, are frequently so arranged as to produce coarse, fine-grained, and other varieties of rocks. These varieties either occur together, side by side, in layers both parallel and regular, and even unequal in size, and variously convoluted; or they are disposed in nodular and spheroidal concretions; or they assume the appearance of veins. All these arrangements vary much in their dimensions and directions: sometimes they are so small as to be confined to individual blocks; but more commonly they extend through several adjacent blocks; and not unfrequently they prevail over considerable masses. In the last case, the layers and veins will be found to affect positions parallel to the principal joints or fissures. Under the same head must be placed the lamellar and slaty structure, as also resulting from a peculiar arrangement of the

constituent particles of rocks. This kind of structure occurs in trap-rocks of all ages, in granite, and in the primary slates, which are also most probably of igneous origin.

The following, therefore, may be the routine by which rocks in a state of fusion have, on cooling, been consolidated. First, the molecules attract each other to form the component mineral or minerals; in the next place, these elementary minerals so unite together as to produce the various concretional forms; and lastly, the whole mass, on becoming solid, acquires certain determinate figures, bounded by planes or joints*. On this view of the subject, the various kinds of rock-structure might be classed under three heads: the molecular, the concretional, and the jointed or crystal-like structures.

That the igneous rocks have been consolidated in the above-mentioned manner, appears to be indicated by the facts, that distinct crystals of the constituent minerals are often intersected both by the concretional and jointed lines of structure; and that, in like manner, the bounding planes of the crystal-like masses traverse the various concretional forms.

I have in another place entered into minute details on this subject, and have adduced the fact of the same jointed structure being reciprocally common both to the granite and the slate at their junction, as additional evidence in favour of the contemporaneous origin of these rocks. The Professor admits (at p. 483.) that the alternate layers of shorl-rock and granite in St. Austel Moor indicate a crystalline arrangement; that "the whole rock has a laminated or veined structure, produced by a peculiar segregation of parts, in passing from a state of fusion into a solid state." Again, "that the alternations of these rocks become more frequent as we approach the junction of the slate, and at last are so frequent and fine-grained, that the rock on the south side of Carclaze becomes finely laminated, and passes into a true schist." And then he asks, "What ought we to infer from a phænomenon like this?" and replies, "that the slate in contact with the granite had at one time been *nearly* in the same condition as the granite; and that both had been modified by a similar crystalline action, in passing into a solid state."

Now, this admission in the case of the granitic rocks of St. Austel Moor, is a most important one towards the confirmation of my views; for if the alternate layers and laminated structure be the result of crystalline action in this in-

* On this particular subject we may refer Dr. Boase and our geological readers to a paper by Mr. Brayley in Phil. Mag. and Annals, N.S., vol. viii. p. 331, in which some of the late Dr. Macculloch's views respecting it are examined and extended.—Edit.

stance, a similar structure in all the other granitic rocks, whether on a larger or smaller scale, must be referred to the same cause. But I subscribe not to the Professor's inference, that the slate and the laminated granite and shorl-rock must have been *nearly* in the same condition; for no evidence can be detected in favour of such a nice distinction to warrant the conclusion that the circumstances under which these rocks crystallized were not perfectly identical. If crystalline action produced the lamination of the granite as well as of the schist, in what respect do these rocks differ from each other, except in the degree of fineness of their laminations? It may be said that they differ in mineral composition and in appearance. I have elsewhere, at great length, attempted to show that in the former respect they do not differ; and in their appearance the difference is not so great as is generally supposed. In the case of Carclaze, just alluded to, there is a great similarity; indeed, there is not nearly so great a difference between the granitic rock and the schist at their junction, as between the former in this position, and the perfectly crystalline rock at a distance therefrom: and the striking resemblance which often occurs between granite and gneiss, and mica-slate, at their contact with each other, needs no remark. Why then, in these instances, is a difference in structure made to establish a distinction of character and of origin? and that too in very opposition to the facts, that igneous rocks do possess the same structure as aqueous formations: even granite, at Arran, having a foliated structure; hornblende-rock, greenstone, and other trappean rocks being frequently schistose; and moreover, the lava of an extinct volcano in Auvergne not only being fissile, but absolutely affording roofing-slates.

After alluding to St. Austel Moor, the Professor adds: "Now all phænomena of this kind accord perfectly with the igneous theory of granite, and its protrusion among stratified slates: yet have they been urged as proofs that the slate rocks of Cornwall (including in the list the fossiliferous slates of Tintagel, &c. &c.) are all contemporaneous with the central granite." This is not a correct representation of my opinion; for the Professor might have known that in the fourth volume of the [Royal Cornwall Geological] Society's Transactions, published in 1832, I virtually retracted the notion that the calcareous series of slates were as old as the granite, by admitting that they contained organic remains; and last year, in my work on "Primary Geology," I excluded this series altogether from the oldest non-fossiliferous slates.

Having said thus much in my own justification, I proceed to consider "the changes which mechanical stratified rocks"

have undergone since their deposition, which is the chief object of the Professor's paper.

These changes he classes under two heads, the chemical and mechanical. The former are illustrated by the globular and concretionary structure of sedimentary rocks, such as the nodular flints in chalk, the globular concretions of calc-grit in oolite, the nodules and spheroidal masses in magnesian limestone, and the balls of nearly pure quartz in the felspathic slates of Wales.

These concretions, with the exception of the last, are certainly instances of a peculiar structure, assumed *since* the deposition of the incoherent materials of which the rocks were formed; but the Professor admits (at p. 467.) that the felspar slates may have resulted from igneous fusion, and "that they ought, in that case, to be removed into another class, and arranged with the orbicular granite of Corsica, and other concretionary trappean rocks." Now, this appears me to be an unnecessary distinction, for the globular concretions of rocks, in formations of all kinds and ages, haveprobably resulted from the same process, viz. the aggregation of their particles during consolidation; as it would seem "that all that is requisite for such an arrangement is, that the integrant particles of the rocks have the necessary degree of motion among themselves during the lapidification; but the manner in which this is attained does not appear to be material."

We are, then, agreed on the fact, that the globular concretions of sedimentary rocks are indications of a change which these rocks have experienced since their deposition; indeed, I have also already stated that "the spheroidal and concentric arrangements of the laminæ in the sandstone at Dunbar have probably been *superinduced* since the accumulation of their original incoherent materials." But we widely differ concerning the cause of these peculiar productions. The Professor mentions, over and over again, that the cause is chemical; whereas I have attributed it to mechanical action, that is, to cohesive attraction. "It appears to be an acknowledged principle," he says, "that when different substances in a state of extreme comminution are mechanically mixed together, they have a tendency to separate and rearrange themselves in masses more nearly homogeneous. The separation of the pounded flint from the aluminous earth, in the materials prepared for the potteries, has been several times quoted as an instance of this kind of chemical action."

Surely this cannot be called an instance of chemical action, in which particles of a like nature, placed at sensible distances, attract each other to form a mass. The definitions of chemi-

cal affinity, and of the attraction of aggregation, very clearly point out the difference between these powers; and they must not, as in the case of geological terms, be treated as "mere affectation"; for definitions are, as aptly styled by Greenough, the standard weights and measures of scientific intercourse.

The Professor next enters on the most important part of his paper, concerning the "transverse cleavage of slate rocks," as deduced from his investigations in Cumberland and Wales. This cleavage he also considers to have been produced by chemical action; and, that it is the only true cleavage, making with the planes of stratification an average angle of from 30° to 40°, being in no instance parallel to the true beds, though sometimes making an angle therewith as low as 10°, and even as 6°. De la Beche has stated that cleavage planes are sometimes at right angles to the strata; but the Professor is of opinion that he has confounded cleavage-planes with joints, of which he treats under a separate head. He also thinks that the Continental geologists have not duly appreciated the transverse cleavage; and that some English writers do not always distinguish between a jointed and slaty structure, but seem to consider a laminated structure parallel to the strata as one of the cases of a slaty structure. These, however, he contends ought never to be confounded; for "they have little in common; can, in ninety-nine instances out of a hundred, be distinguished even in hand-specimens; and ought to be designated by separate names."

This supposed facility of drawing distinctions between the two kinds of structure has induced the Professor to offer definitions of them, notwithstanding his wonted dislike to such labours. "Before I conclude this section," he observes at p. 479, "I cannot help recommending, not a new nomenclature, but a more systematic use of old terms than we are accustomed to. *Bed* is always applied as the English synonym of *stratum*, and the terms *thick-bedded*, *thin-bedded*, *thick-flaggy*, *thin-flaggy*, and *laminated*, are words in common use, and express well enough different modifications of stratified structure. The term *foliated*, again, expresses very well the peculiar structure of mica-schist, and the fine glossy undulating layers of greywacké. But it would be well to describe no structure as *slaty* or *fissile* except cases of transverse cleavage, using the term *slate* for a perfect oblique cleavage, and some such term as *flagstone-slate* for imperfect cleavage; and in like manner *slaty-flagstone* may describe a very thin or laminated structure, parallel to the stratification. In this way, *foliated* as distinct from *laminated*, and *slaty* as distinct from *flaggy*, become terms of a definite meaning."

Now, let us examine into the practical working of this "more systematic use of old terms." We will take, in the first place, "hand-specimens" of the very fine felspar, actynolite, hornblende and chlorite schists; or of the Delabole slate near Camelford, which is perfectly fissile in the usual sense of the word, affording roofing-slates second to none in the universe. Are these *slates*? Have they the *slaty structure*? Or, in other words, do they possess a *perfect* cleavage? The Professor has said that this kind of structure ought never to be confounded; and yet it is impossible to answer this question, according to the proposed system, without having previously ascertained the direction of the strata. It is therefore evident that the slaty structure cannot be distinguished "in ninety-nine cases out of a hundred, even in hand-specimens." Perhaps this example will be objected to as a Cornish case—the one in a hundred—an exception to the general rule, which may be discarded as unessential, according to another canon of the Professor, which (at p. 481.) runs thus: "It is the business of a geologist to consider both the resemblances, and the differences of the things he describes; and after a broad view of nature's kingdoms, he learns to seize upon those resemblances which are essential to his classification, and to cast from his thoughts those differences which are unessential." This is certainly a royal road to geology, a clue to the labyrinths of geological theories.

But to return. If examples of Cornish slates be disliked, take *hand-specimens* of the greywacké roofing-slates of Cumberland and Wales. Are these *true slates*? Undoubtedly not, according to the new system, for we are informed (at p. 474.) that "these thin laminæ often resemble the coarser varieties of slate, and indeed are sometimes used for the same purposes, but they are only *flagstones* or thin beds." How is this determined? By the same rule as denied to the Cornish schists the privilege of being slates, viz. parallelism to the strata; for which reason, it is also said (at p. 479.) that "the oldest and most crystalline rocks, designated by the general name of schists, have no true slaty cleavage, in the sense in which I have used the term. The value of this rule may be better appreciated, as conferring a "definite meaning" on the term *true slaty cleavage*, when it is remembered that the Professor will not allow the word stratum to be susceptible of an accurate definition.

[To be continued.]

XLIX. *Reviews, and Notices respecting New Books.*

On the Solution of Numerical Equations: translated by W. H. Spiller, *from a Memoir of* M. C. Sturm, *presented to the Royal Academy of Sciences of Paris.* 4to. J. Souter, London; Bachelier, Paris.

THE *mémoire*, of which the present publication of Mr. Spiller is a translation, was printed in Paris about three months ago, in the sixth volume of "Mémoires présentés par des Savans etrangers." It forms the most valuable addition to our knowledge of the theory of numerical equations that has been made since the time of Lagrange; in fact, it fully accomplishes the great object which, from Descartes till now, has engaged the thoughts of mathematicians in this department of analysis, and which is no other than the complete determination of the number and situation of the real roots of every numerical equation, a problem to which the labours of Lagrange were, as is well known, especially directed. The success of these labours, however, extended no further than to show that the solution of the problem in question was mathematically possible, although practically nearly impossible, on account of the mass of numerical labour, and the consequent risk of error, involved in the method which he proposed.

As a means, therefore, of facilitating the actual evolution of the real roots of numerical equations of the higher order, the process of Lagrange was entirely useless. Very recently, however, the researches of Budan and Fourier have proved that *the rule of signs* of Descartes was far more comprehensive than had hitherto been supposed; and that by employing it in its full extent, it was capable of furnishing much more explicit information respecting the nature of the roots of an equation than had as yet been expected from it. Still these additional advantages did not reach the length of supplying us with the exact number of real roots comprised between any two proposed limits, and yet nothing short of this knowledge would suffice fully to answer the demands of the practical computer.

It is true that the last-named mathematician, Fourier, in his posthumous work entitled "Analyse des Equations déterminées," has pushed his inquiries beyond those of Budan, and has added to the rule of Descartes a process of his own for ascertaining the nature of those numerical intervals which that rule left in doubt; he has therefore the merit of having thus supplied the deficiencies of Descartes' rule; and there can be no question that the combination of this rule as improved by Budan, with the supplementary process of Fourier for ascertaining the precise character of the doubtful intervals, is sufficient to make known the nature and situation of the roots of a numerical equation, without having recourse to the method of the squares of the differences. Still the method of Fourier is slow and tedious, tentative and inelegant; and is not, after all, capable of making known *à priori* the exact number of the real and imaginary roots, without proceeding to the actual solution of the equation. Now it is a remarkable peculiarity in the theorem of Sturm that it completely solves the problem by a perfectly in-

dependent and singularly simple process, altogether preparatory to the actual evolution of the roots themselves. So simple and so satisfactory is the method of Sturm, that it must entirely supersede all former investigations on this subject, and it will henceforth occupy the same distinction in the theory of equations that Taylor's theorem does in the differential calculus. The present translation, from a large and expensive volume not readily accessible in this country, cannot fail to be acceptable to all classes of readers interested in the progress of mathematical science. It seems to have been executed with great care and fidelity, and is, indeed, superior to the original in one important particular—typographical accuracy; since, upon a careful comparison of the two, we find that several errors in the original have been corrected in the translation. The work is printed in a manner worthy of the subject. ***

L. *Proceedings of Learned Societies.*

OFFICIAL REPORT OF THE PROCEEDINGS OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, AT THE DUBLIN MEETING, AUGUST 1835.

Communicated by the Council and Secretaries.

[Continued from p. 315.]

Notices and Abstracts of Miscellaneous Communications to the Sections, continued.

CHEMISTRY.—ELECTRICITY.

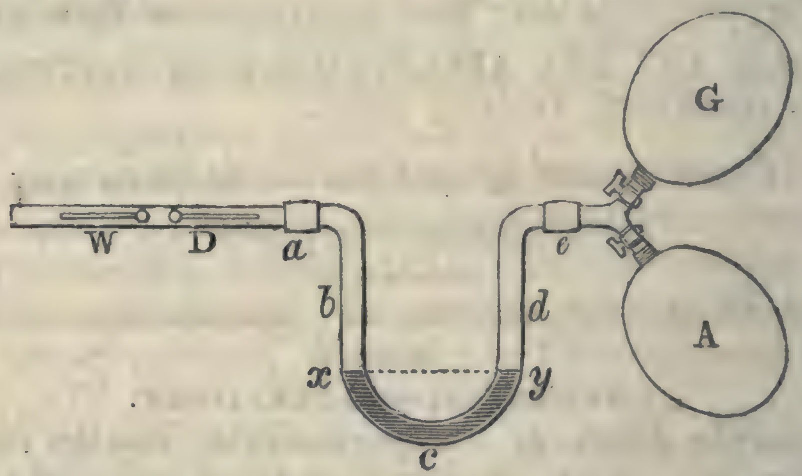
On the Specific Heats of the permanently elastic Fluids. By James Apjohn, *M.D., Professor of Chemistry in the Royal College of Surgeons, Ireland.*

After an introductory view of the state of knowledge on this subject, Dr. Apjohn proceeded to explain the principle of an entirely new method which he was enabled to apply to the investigation of the difficult problem under consideration, in consequence of having been recently fortunate enough to arrive at a formula which expresses, with extreme and unexpected precision, the relation existing between the indications of a wet-bulb thermometer and the corresponding dew-points. This formula* (see Proceedings of Mathematical and Physical Section, p. 313,) being equally true of all gases, obviously suggests a method of comparing their specific heats. For as in the case of every gas it may be deduced that $a=\frac{(f'-f'')e}{48\,d}\times\frac{30}{p}$, it is clear that if we determine values of f', f''

* $f''=f'-\frac{48\,a\,d}{e}\times\frac{p}{30}$, in which f'' is the elastic force of vapour at the dew-point; f' its elastic force at the temperature t', shown by the wet thermometer; d the difference between the latter temperature, and t that of the air; a the specific heat of air; and e the caloric of elasticity of the vapour of water whose elastic force or tension is represented by f'.

and d in different elastic media, we shall have data for ascertaining their relative capacities for caloric. Such a method, however, though theoretically exact, is beset with difficulties so great that it may be considered as practically impossible. The artificial gases, as usually collected, are saturated with moisture, a state in which they are quite unsuited for the necessary experiments; and even though this difficulty were overcome, it would probably be impossible to determine their dew-points by direct experiment.

If, however, we suppose that $f''=0$, or that the gas is perfectly dry, the above value of a will become $\frac{f' e}{48\ d} \times \frac{30}{p}$, an expression involving no unknown quantities but f' and d, and which will therefore enable us to calculate the specific heat of a gas when we have observed the stationary temperature t', to which, when in a state of perfect desiccation, it brings the wet-bulb thermometer. In order to the determination of t', and of $t-t'=d$, the following method of experimenting was, after a trial of several others, finally adopted.



Into a bent tube, *a b c d e*, about 50 inches long, and $\frac{2}{10}$ths of an inch in diameter, oil of vitriol was poured to the height marked by the horizontal line *x y*, and to one extremity of this siphon a pair of bladders furnished with stopcocks were attached, through the intervention of a three-armed copper pipe, while to the other extremity of the apparatus there was connected by a caoutchouc collar a glass tube carrying the dry thermometer D, and wet one W. Matters being thus arranged, an assistant pressed, by means of a deal board, first upon the bladder A, containing atmospherical air, and, when it was exhausted, upon the bladder G, containing the gas which was the immediate subject of experiment. The air, in passing through the oil of vitriol, was deprived of its vapour, and in subsequently traversing the tube containing the thermometers, produced in the *wet* one such a reduction of temperature, that, upon continuing the experiment as rapidly as possible with the *gas*, the wet thermometer soon acquired a stationary temperature,—which, when attained, was, as well as the indication of the dry instrument, carefully noted. The residual gas was now passed into a glass jar on the mercurial trough, with a view to a subsequent analysis; and both bladders being refilled with atmospherical air alone, a second experiment was performed precisely as just described.

From the values of t 0 t' obtained in the first experiment, we ob-

tain, by aid of the equation $a=\frac{e\,f'}{48d}\times\frac{30}{p}$, the specific heat of the elastic fluid which was made to traverse the apparatus. But this result belongs not to the pure gas, but to a mixture of it with a certain quantity of atmospheric air, which entered the bladder upon the principle of endosmose; and to infer from it the specific heat of the pure gas, which we shall call x, it was necessary to know, 1st, the amount of air present, and 2nd, *its* specific heat. Now the former of these data was given by the analysis of the residual gas, as already mentioned, and the latter by the results of the second experiment above recorded, in which both bladders were occupied by air alone. If x be the specific heat, and s the specific gravity of the gas, n the per centage of air, c its specific heat, and a the specific heat of the mixture of air and gas, as already determined, we shall, on the principle that the specific heat of a mixture multiplied by its weight is equal to the sum of the products of the weights of the gases mixed multiplied by their respective specific heats, have $x(100-n)s+n\,c=a(\overline{100-n\,s+n})$, an equation from which we deduce $x=a+\frac{(a-c)\,n}{(100-n)\,s}$. This is the specific heat of the pure gas in reference to that of air, as determined by the second of the above experiments; and as both air and gas are dry, and must have been, with at least a very high degree of probability, proportionally affected by variations of pressure, the precise influence of these, about which, indeed, philosophers are not agreed, do not require to be taken into consideration; nor is there anything further necessary for rendering the result thus obtained strictly comparable with those of other experimenters, than to reduce it, by the rule of three, to what it would be if the specific heat of air were ·267, the number by which it is usually represented in books at a mean altitude of the barometer. The following experiments on air and hydrogen, performed on the 4th of August, will illustrate the preceding description.

	t	t'	d	p
Hydrogen......	68	48	20	30·114
Air...........	68	43	25	30·114

By applying to these results the equation $a=\frac{f'e}{48\,d}\times\frac{30}{p}$, we get

Specific heat of air .... =·2767
Approximate specific heat of hydrogen . =·4092

But the gas, upon analysis, was found to contain 5 per cent. of air. Hence the specific heat of the hydrogen supposed pure as deduced from the equation $x=a+\frac{(a-c)\,n}{(100-n)\,s}$ becomes ·5097. And ·2767 : ·5097 : : ·2670 : ·4914 = the specific heat of hydrogen compared to that of air under a pressure of 30,—when water is represented by unity, or, what amounts to the same, when air is ·267.

The following table exhibits the results thus obtained;—referred to air as the standard; the number for nitrogen being the mean of

two; that for hydrogen of four; that for carbonic oxide of three; that for carbonic acid of three; and that for nitrous oxide of two experiments.

	Specific Heats of equal volumes.	Specific Gravities.	Specific Heats of equal weights.
Air.............	1·0000	1·0000	1·0000
Nitrogen.........	·9613	·9722	·9887
Hydrogen........	·1315	·0694	1·8948
Carbonic oxide....	1·0508	·9722	1·0808
Carbonic acid.....	1·6677	1·5277	1·0916
Nitrous oxide.....	1·7802	1·5277	1·1652

A bare inspection of this table would seem to justify the conclusion that, with a single exception, the different gases operated with have, under equal volumes, specific heats proportional to their specific gravities; and of course that, under equal weights, they have the same specific heat. In the excepted case, that of hydrogen, the specific heat is nearly the double of that which would result from this law.

On the absence of Magnetism in Cast Iron when in fusion. By R. W. Fox.

In the course of some magnetic experiments, it appeared to the author desirable to ascertain whether a magnetic needle is acted upon by cast iron in a state of fusion. For this purpose he had a horizontal mould made in sand, about five feet long and two inches square, in the direction of the magnetic meridian; and at a very small distance from its northern extremity, and parallel to it, he placed the south pole of a delicately poised magnetic needle, the north pole of which extended beyond the mould. The latter was then filled with very fluid melted iron, but not the slightest effect was produced on the needle till after the metal had become fixed and cooled down to a cherry-red colour. The needle was then very suddenly attracted with great energy. Sand and a copper plate were employed to protect it from the hot iron.

This experiment may perhaps be considered by those who advocate the existence of a high temperature in the interior of the earth, as tending to strengthen the arguments in favour of the agency of electricity in producing terrestrial magnetism, seeing that intense heat and fixed magnetism, in the ordinary acceptation of the term, cannot, apparently, exist together.*

On Electric Currents passing through Platinum Wire. By William Barker, M.D.

When the large deflagrating battery was used on two occasions

* [Mr. Peter Barlow had shown, many years since, that all magnetic action was lost by iron when raised to a white heat; from which the inference above drawn by Mr. Fox has, we believe, been deduced by other men of science; but we are not aware that the experiment was ever tried before with iron in a state of fusion. See Phil. Trans. 1822, or Phil. Mag., first series, vol. lx. p. 345.—Edit.]

in the Chemical School of Trinity College, Dr. Barker observed that on passing the current of electricity through a piece of platina wire, about three feet in length, and igniting it, there were dark portions of the wire of about $\frac{1}{4}$ or $\frac{1}{2}$ an inch in length at intervals of from three to four inches in its whole length, the same parts of the wire being in the same condition during the time that the wire remained ignited. The author was unable at the time, owing to the number of experiments to be tried with the battery, to take any measurements or to examine whether wires of different diameters were differently affected. The fact was stated for the consideration of the section, reserving for further examination the law by which the distances and dimensions of these unignited portions of the electrified wire are governed, whether their distances are constant, or vary according to the size and material of the wire employed or the quantity or intensity of the galvanic currents.

An Account of some Experiments recently made on the Buoys in Kingstown Harbour, with a view to protect from the action of Sea Water the Metals, and especially the Iron-work, attached to them. By Edmund Davy, *F.R.S., M.R.I.A., &c., Professor of Chemistry to the Royal Dublin Society.*

Last year an enlightened member of the Royal Dublin Society, Mr. John M'Mahon, made the author acquainted with the fact that the iron-work attached to the new buoys lately put down in Kingstown Harbour had undergone a very rapid corrosion by the action of sea-water on it; and shortly after, the Commissioners of Public Works acting as Commissioners of Kingstown Harbour directed his attention to the subject, with the view of ascertaining the cause of such corrosion, and the means of prevention.

The new buoys* are precisely similar to the buoys, of the most approved construction, now used in Portsmouth Harbour. The whole surface of each buoy is sheathed with copper, except the bottom and about three inches of the smaller end, which is covered with lead, fastened to the copper by metal nails. A bolt passes through the whole length of the buoy, and is terminated at each end by a shackle. The lower shackle has a bridle patent chain fastened to it by means of a bolt and a thin pin called a *forelock*, which is such an important part that on its preservation mainly depends the security of the ships moored to the buoy†. The bridle chain is secured to a larger chain-cable and moorings, by means of shackles, bolts, and forelocks. The forelocks require to be examined about once a year, and replaced if defective. The bolt, shackles, chains, and forelocks are all of the best wrought iron.

On examining the buoys the author found all the iron-work at and near their bottoms very much corroded; and the corrosion ap-

* The author exhibited a drawing of one of these buoys.

† Some years since the Lord Lieutenant's yacht broke from her moorings in Kingstown Harbour in consequence of the defective state of the forelock.

peared to be most considerable on the iron in the immediate vicinity of the lead, where it was about one eighth of an inch deep, and the metal was so much indented as to exhibit a coarse fibrous structure. So rapidly had the iron-work corroded in about six months, that had it continued at the same rate for two years the buoys (in the opinion of competent judges) would have been quite unfit for the public service. The copper and lead attached to the buoys were in a good state of preservation.

The extraordinary corrosion of the iron-work appeared to be due to an electrical action produced in sea water by the contact of the iron with the lead joined to the copper, on the buoys; these metals being preserved at the expense of the iron. The author submitted his views on the subject to the Commissioners, and suggested the propriety of removing a circle of about three or four inches of lead from the iron-work at the bottom of each buoy, and of driving two or three short large-headed iron nails through the remaining lead into the wood, in order to protect both the lead and copper covering of the buoys from corrosion. These suggestions being promptly carried into effect, the author has during the last twelve months had frequent opportunities of examining the state of the iron-work attached to, and in the immediate vicinity of, the buoys, and he states that the removal of the lead has put a stop to the very rapid corrosion of the iron-work.

The action of sea-water on iron, under ordinary circumstances, is, as is well known, by no means inconsiderable. The author found that a piece of iron chain weighing 14 pounds 5 ounces, when exposed for 24 hours in $5\frac{1}{2}$ quarts of sea-water, lost 70 grains, and in a few days upwards of a quarter of an ounce: these facts led him to think it both desirable and practicable to coat the iron-work of the buoys, &c. with a varnish or japan which should be impervious to sea-water: and at the request of the Commissioners he made many experiments, using different varnishes and japans; but the results obtained were for the most part of a negative kind, owing not only to the action of sea-water on iron, but also to the constant friction to which the metal must be exposed, from the unceasing influences of tides, winds, and the strains from ships. He has hitherto found no varnish or japan that he can recommend as a means of preventing, for any length of time, the ordinary corrosion or oxidation of iron in sea water.

The author made a number of experiments with a view to apply metallic protectors to the iron-work connected with the buoys, on the principle developed by the late Sir H. Davy. He found that when small ingots of zinc were attached to pieces of chain cable in sea-water, during several weeks, these lost no weight, and the corrosion of the zinc was inconsiderable. Hence it seemed obvious, that zinc will protect iron from corrosion in sea-water. These results were so satisfactory that the author recommended the experiments to be tried on the buoys, and the Commissioners immediately requested him to carry the same into effect. He has had under a course of trial for several months, in contact with the iron-

work at the bottom of each buoy, two zinc protectors, each of which is about 6 inches long and $\frac{3}{4}$ inch wide, and weighs about 8 ounces; and on a recent examination, the iron-work near the zinc exhibited a clean appearance. There is another and a still more recent application of the zinc, which the author thinks will be very beneficial in protecting a most important part of the iron-work already alluded to, namely, the *forelock.* Several of the forelocks have stout zinc rings cast into holes made in their heads, and on lately examining a forelock so protected for several weeks, it was found quite free from corrosion.

The late Sir H. Davy referred the corrosion of copper in sea-water to the agency of the oxygen of the air. The author from his experiments has obtained results which lead to the same conclusion with regard to iron. He found also that the corrosion of iron in sea-water is materially influenced by the depth of water in which the metal is immersed. He is of opinion that the wear of iron-work exposed to sea-water is more considerable the nearer the iron is to the surface or to the external air. The principal wear of the iron-work connected with the buoys seems to be at and within a few feet of the surface of the water; and this portion of the iron may be protected by attaching strong pieces of zinc to it.

The corrosion of iron in sea-water, under ordinary circumstances, appears to arise from exposure of the water to the atmosphere, and the consequent gradual absorption of its oxygenous part. The protection of iron in sea-water by the contact of zinc seems due to a simple electrical action between the respective metals and the fluid; water being decomposed, its hydrogen is evolved, its oxygen goes to the zinc, whilst the oxide of zinc as it forms seems to be deposited on the iron, at least in part.

The author made a number of experiments to ascertain whether zinc would protect iron in sea-water if a very thin surface of glass, wood, paper, tow, &c. were severally interposed between those metals, but the results seemed clearly to prove that actual contact of the metals is indispensably necessary to that effect.

Zinc will protect iron in fresh water. The author has made experiments on this subject, and has others still in progress; the results of which may admit of useful applications to valuable parts of machinery, &c.

The author expressed his obligations to Mr. Hutcheson, the Harbour-Master at Kingstown, for the kind and prompt assistance he afforded on every occasion, and for the interest he took in the progress of the experiments on the buoys, &c.

On some recent Experiments made with a view to protect Tin Plate or tinned Iron from corrosion in Sea-water, with some probable applications; and on the power of Zinc to protect other Metals from corrosion in the Atmosphere. By EDMUND DAVY, *F.R.S., M.R.I.A., &c., Professor of Chemistry to the Royal Dublin Society.*

If a piece of tin plate is exposed in sea-water for a few days, it

will exhibit an incipient oxidation, which will gradually increase; the tin will be preserved at the expense of the iron, which will be corroded. But if a small surface of zinc is attached to a piece of tin plate and immersed in sea-water, both the tin and iron will be preserved, whilst the zinc will be oxidated, on the principle first made known by the late Sir H. Davy.

The author has exposed for nearly eight months in sea-water a surface of tin plate nailed to a piece of wood by means of tinned iron tacks, inserting between the wood and the tin plate a small button of zinc. Under these circumstances the tin plate has remained clean and free from corrosion; the zinc has of course been corroded. In a comparative experiment, in which a similar piece of tin plate was nailed to the same piece of wood, and exposed during the same period to the same quantity of sea-water, without the zinc, the edges on two sides of the tin plate were quite soft from the corrosion, which had extended to about $\frac{1}{8}$th of an inch. These experiments seem worthy of being repeated and extended.

The present demand for tin plate is very great; should these statements be confirmed, a vast increase in its consumption might be anticipated. The opinion may be entertained that it is practicable to substitute double tin plate for sheet copper in covering the bottoms of ships, &c., using zinc in small proportion as a protector. Such applications would probably occasion a saving of nearly three fourths of the present expense of copper sheathing.

It also seems deserving of inquiry whether tin plate vessels, protected by zinc, may not be advantageously substituted for copper vessels in many of our arts and manufactures, and even in domestic œconomy. Although it might be presumed from Sir H. Davy's experiments and observations* that zinc would protect tin plate from corrosion in sea-water, the author is not aware that any direct experiments on the subject have been published. Sir H. Davy briefly refers to some obvious practical applications of his researches to the preservation of finely divided astronomical instruments of steel by iron or zinc; and that Mr. Pepys had taken advantage of this last circumstance in inclosing fine cutting-instruments in handles or cases lined with zinc. The author has not heard whether such applications have succeeded, but he has made a number of experiments with a view to protect brass, iron, copper, &c. from tarnish and corrosion in the atmosphere by means of zinc; the results obtained, however, lead to the conclusion that contact with zinc will not protect those metals in the atmosphere, the electricity thus produced, without the intervention of a fluid, being apparently too feeble to counteract the chemical action of air and moisture on the surfaces of these metals.†

* Phil. Trans. vol. cxiv. for 1824 [or Phil. Mag., first series, vol. lxiv. p. 30, 233; vol. lxv. 203.—Edit.]

† [The negative results thus obtained by Mr. E. Davy, agree exactly with those of some trials which I have witnessed for protecting steel by this means.—E. W. B.]

On the comparative value of Irish and Virginian Tobacco. By EDMUND DAVY, *F.R.S., M.R.I.A., &c., Professor of Chemistry to the Royal Dublin Society.*

In the year 1829–30 the cultivation of tobacco in Ireland excited much attention among agriculturists, and several hundred acres of it were raised in different counties; in consequence, the attention of the Royal Dublin Society was directed to the subject, and the author was requested by a select committee of that body to institute experiments on tobacco with a view to determine some questions of a practical nature, as whether its root contained nicotin, and in what quantity, and to ascertain the comparative value of Irish and Virginian tobacco.

The author's experiments were made on average samples of Virginian and Irish tobacco; for the former he was indebted to the kindness of Mr. Simon Foot, and for the latter to Messrs. Wild, Cuthbert, Callwell, and Brodigan. From a number of experiments the author was led to conclude that the dried roots of Irish tobacco contain from four to five parts of nicotin in one hundred parts; and that one pound of good Virginian tobacco is equivalent in value to about 2¼ pounds of good Irish tobacco.

After the author had finished his experiments it was gratifying to him to be informed that some manufacturers estimate one pound of Virginian tobacco as equivalent in value to about two pounds of Irish. Hence there seems to be a pretty near coincidence between their results and those derived from a chemical examination.

On Nicotin and some of its Combinations. By EDMUND DAVY, *F.R.S., M.R.I.A., &c., Professor of Chemistry to the Royal Dublin Society.*

When the author commenced his experiments in 1829 on Irish and Virginian tobacco, nearly all our knowledge of the peculiar principle in tobacco, called *Nicotin* by the late M. Vauquelin, was confined to his paper on tobacco*. By a series of processes in which the expressed juice of tobacco was reduced to one fourth of its bulk by evaporation, then digested in alcohol, distilled, again concentrated, dissolved in alcohol, then evaporated to dryness, dissolved in water, saturated with potash, and distilled to dryness, Vauquelin seems to have obtained a fluid nearly approximating to the nicotin recently procured.

In obtaining nicotin, the author avoided the circuitous processes of Vauquelin, and adopted only the simple method of exposing tobacco to the action of a solution of potash and subsequent distillation. The alkali employed was in some cases weak and in others strong. In some instances it was macerated on the tobacco for one or two days; in others, it was added to the tobacco in the retort and distilled at once. Other fixed alkaline substances in solution, as soda, barytes, strontites, lime, may be substituted for potash. Distillation was occasionally carried on below, but in general at the

* *Annales de Chimie*, tome lxxi.

boiling point. Under such varied circumstances, the fluid procured, on being rectified by a second distillation, is an aqueous solution of nicotin, having the following properties. It is colourless and transparent. Its odour closely resembles that of tobacco, but is far more pungent. Its taste is peculiar, and leaves a sharp biting impression on the tongue for some time. It changes turmeric paper to brown; but this effect is not permanent, but gradually disappears on exposure to the air. Its specific gravity (according to repeated trials made by two intelligent pupils of the author, Mr. Richard Austin and Mr. John Keogh, who assisted him in many of his experiments,) is about that of distilled water. It neutralizes the mineral and vegetable acids, forming peculiar salts, some of which the author has obtained in a crystallized, and others in an imperfectly crystallized state. It undergoes no apparent change by being kept in close vessels for a considerable length of time. It is volatile below the point of boiling water. It precipitates the greater number of metals from their solutions, as those of silver, mercury, tin, antimony, manganese, of a white colour; iron of a green, cobalt of a pink, and gold and platina of a yellow colour.

Salts of Nicotin.—A number of the salts of nicotin, as the nitrate, sulphate, &c., crystallize in four- and six-sided prisms; they are characterized by having a sharp biting taste, analogous to that of aqueous nicotin: they are mostly soluble in water, and are easily decomposed by a slight increase of temperature. The nitrate is so susceptible of change, that it seems to undergo an incipient decomposition when exposed in solution for a few hours, and assumes a reddish colour. The author's experiments have led him to conclude that nicotin is composed of carbon, hydrogen, oxygen, and nitrogen, but he is not yet satisfied as to its exact constitution. He made some experiments to try the effects of aqueous nicotin on small fishes, flies, moths, spiders, &c. A few drops of it diffused in a tumbler of water strongly acted on the nervous system of small fishes, immediately communicating to them an unusual but momentary energy, which was speedily followed by torpor.

Butterflies, moths, spiders, were soon killed by being brought in contact with a weak solution of nicotin. Common flies resisted its action better than spiders, drones, bees, or wasps, and after immersion for a short time, again recovered on being exposed to the air for a few minutes. Common caterpillars of a large size, on being taken from cabbages, and instantly put into a weak solution of nicotin, exhibited some energy, but presently became insensible, and being considered as dead were suffered to remain in the solution for about half an hour; they were then removed to fresh water, but exhibited not the slightest symptoms of life, but on being placed on a grass plot near the house they all recovered, and were very active in the course of an hour.

The author is of opinion that aqueous nicotin may admit of a number of useful applications, as in preparing specimens of natural history for the museum, in preventing the destructive effects of the insect tribes which infest plants and trees in gardens, conservatories,

&c. And it seems highly probable that the salts of nicotin will admit of useful medicinal applications.

After the author had ascertained the principal facts already stated respecting nicotin, he found that he had been anticipated; he observed in the 'Quarterly Journal of Science, Literature, and Art,' for December 1829, that MM. Posselt and Reimann had lately obtained a vegeto-alkali from tobacco, examined its properties, and combined it with a number of acids. But though those chemists are justly entitled to the merit of having first made known to the public an interesting series of facts respecting nicotin, the author's experiments may serve to corroborate their general results, and also throw additional light on the subject.

On a peculiar Fluid obtained in the manufacture of Pyroxylic Spirit. *By* M. Scanlan.

The author has been for some past engaged in the making of pyroxylic spirit*, a fluid now extensively used in England as a substitute for alcohol, principally by hat manufacturers, for the purpose of dissolving shell lac and mastic to stiffen their hats and render them water-proof.

In the process which Mr. Scanlan pursues, he obtains a fluid of a higher specific gravity, but having a lower boiling point than pyroxylic spirit, and differing from it in other respects.

Rough pyroligneous acid is submitted to distillation in a copper still, by the maker, in order to separate some of the tar it holds in solution; he sets apart the first 15 per cent. that distils over, and this he sells as wood spirit. This liquor, as it comes from the pyroligneous acid maker, contains much free acetic acid and tarry matter.

The author proceeds to saturate the acetic acid by means of slacked lime, which causes the separation of some pitch.

He next submits the saturated liquor to distillation as long as the distilled product is of less specific gravity than water.

This last product is rectified in a still somewhat on the plan of those for a long time in use on the Continent, and now coming into general use in this country, for the purpose of rectifying spirit. It consists of a boiler, containing the liquor submitted to distillation, and of a rectifier, which is a copper vessel of peculiar construction, placed in a bath of water, which must be kept at such a temperature as will condense water, but still retain the more volatile products in the state of vapour till they pass into the last part of the apparatus, where they are condensed and finally cooled.

In this process of rectifying, the author was a good deal sur-

* It is known by the name of naphtha by those who deal in and consume it. [This fluid was discovered in 1812 by Mr. Philip Taylor, who published an account of its properties, giving it the name of pyroligneous æther, in 1822. See Phil. Mag., first series, vol. lx. p. 315. In the following year it was examined by MM. Macaire and Marcet, by whom it was termed pyroxylic spirit. See *Bibliothèque Universelle* for October, 1823; and also a paper by MM. Dumas and Peligot, *Annales de Chimie et de Physique*, tome lviii. p. 70, or our present number, p. 427.—Edit.]

prised to find the product first condensed had a higher specific gravity than that which succeeded to it in the distillation. The first being about ·900, and the second so low as ·830; to this, if the distillation be pushed far enough, succeed water and an oil which becomes black by keeping. The fluid having specific gravity ·900, is a good deal coloured; treated with animal charcoal its colour is removed; rectified from a water bath after treatment with animal charcoal, its specific gravity is ·911, and its boiling point about 132°.

In this state it is colourless and inflammable: it has a powerful, and to most persons a very disagreeable smell. Caustic potash decomposes it instantly, acetate of potash being formed, and probably carbonate of potash. It forms acetate of lime also when slacked lime is added to it. It softens copal, but dissolves very little of it. When diluted with water it does not comport itself as alcohol of the same specific gravity does; 50 measures of it mixed with 50 of water at the temperature of 54, were raised in temperature to 61, and a considerable quantity of air was extricated; the mixture brought again to the temperature of 54, measured but 96·5 measures, and its specific gravity was ·9861. Alcohol diluted so as to have specific gravity ·911, when similarly treated, measured 98, and its specific gravity was ·9659.

Litmus paper immersed in it is not reddened, but on exposure to the air the fluid evaporates and leaves the paper permanently red.

It mixes with water in every proportion, and water may be separated from it by means of carbonate of potash as from dilute alcohol, but which is not the case with pyroxylic spirit *.

On the chemical constitution of Fossil Scales, as illustrative of the nature of the Animals from which they have been derived. By Arthur Connell.

The difficulty of determining merely from external characters whether a fossil scale has belonged to a fish or to a saurian animal, and the geological interest which that problem frequently possesses, render it desirable to know whether chemical means are capable of solving it.

Mr. Hatchett ascertained that the scales of recent reptiles consist chiefly of a horny substance, whilst those of fish contain a considerable proportion of phosphate of lime, and are of the nature of bone. Chevreul confirmed his observation as to fish scales; and the author has found that the scales of small recent crocodiles contained little more than one per cent. of incombustible earthy matter, although in the carinated dorsal scales the amount extended to about 3 per cent. When fish scales are fossilized we may therefore expect that the bone earth will remain, and the perishable animal substance will either disappear without any substitution, or be wholly or in part replaced by siliceous or calcareous matter; whilst, on the other hand, if a saurian

* [There appears to be great reason to doubt whether the fluid here described by Mr. Scanlan, be not merely *pyroacetic spirit*, retaining acetic acid as an impurity.—Edit.]

scale is mineralized it ought to consist almost entirely of some replacing substance, such as siliceous or calcareous matter, coming in place of the decaying animal matter and of little or no bone earth.

The author has analysed fossil scales from the three following localities, and the result of the analysis he conceives to show the whole of them to have belonged to fish:

	Burdie-house.	Craighall Coal.	Tilgate.
Phosphate of lime	50·94	55·75	60·13
Carbonate of lime	11·91	15·86	27·94
Siliceous matter	36·58	16·17	3·42
Potash and soda	·47	1·06	1·43
Alumina	......	2·82	·82
Bituminous matter and water*	·12	6·46	6·71
Phosphate of magnesia	Trace.		
Animal matter	Trace.		
	100·12	98·12	100·45

In the first of these the animal matter appears to have been replaced by siliceous matter; in the two others, partly by siliceous matter, and partly by carbonate of lime.

The author has had no opportunity of examining an undoubted saurian fossil scale.

On the Composition and Properties of the Salts of Sulpho-Methylic Acid. By Robert J. Kane, *M.D., M.R.I.A.*

Professor Kane had been occupied with experiments on pyroxylic spirit, in order to test the truth of Liebig's idea of its nature, and had announced to the Royal Irish Academy the fact of the formation of a peculiar acid, analogous to the sulphovinic, by the action of sulphuric acid, before he received an account of Dumas and Peligot's researches on that substance. The question of its nature having been decided by their analysis, he restricted himself subsequently to the development of the history of the sulpho-methylates, a department of the subject on which the French chemists had but slightly touched.

The sulpho-methylates are easily prepared. A salt of lead may be procured by mixing pyroxylic spirit with an equal weight of oil of vitriol, and neutralizing by carbonate of lead. It crystallizes in fine long rectangular prisms. A salt of baryta can be obtained in a similar manner with carbonate of baryta. From either of these salts the other sulpho-methylates can be obtained, by double decomposition, by means of a soluble sulphate.

The sulpho-methylate of potash crystallizes in pearly rhomboidal plates; it deliquesces. Heated it gives water, neutral sulphate of methylene, and sulphurous acid, leaving a carbonaceous

* The Tilgate scales contained carbon and sulphur instead of bituminous matter.

residue of sulphate of potash. The mean of three analyses gave for its composition,

Potash	29·51
Sulphuric acid	50·10
Methylic æther	14·39
Water of crystallization	6·00
	100·00

which agrees with the formula:

$$\overline{2\dddot{S}+\dot{K}+(2C+3H+O)}+\dot{H}$$

The sulpho-methylate of baryta crystallizes in plates. The mean of two analyses gave

Baryta	38·50
Sulphuric acid	40·21
Methylic æther	11·49
Water of crystallization	9·80
	100·00

This salt was analysed by Dumas. His result:

Baryta	38·6
Sulphuric acid	40·4
Methylic æther	11·1
Water of crystallization	9·9
	100·0

Both analyses indicate the same formula:

$$\overline{2\dddot{S}+\dot{B}a+(2C+3H+O)}+2\dot{H}$$

The sulpho-methylate of lime crystallizes in octohedrons, which are anhydrous. They deliquesce, and by the mean of two analyses are composed of,

Lime	21·41
Sulphuric acid	60·25
Methylic æther	18·34
	100·00

giving the formula $2\dddot{S}+\dot{C}a+(2C+3H+O)$.

The sulpho-methylate of lead usually obtained is in long prisms, which readily deliquesce, and are very easily decomposed, being resolved into sulphate of methylene and sulphate of lead. The mean of seven analyses of this salt gives for its composition,

Oxide of lead	49·76
Sulphuric acid	35·93
Methylic æther	9·81
Water of crystallization	4·50
	100·00

giving the formula $2\dddot{S}+\dot{P}b+(2C+3H+O)+\dot{H}$.

On two occasions a lead salt was obtained in plates like the baryta salt, and apparently containing two atoms of water; but Professor Kane has not determined the exact circumstances necessary to the production of this form, and consequently its examination yet remains to be made.

The salts of copper, nickel, soda, ammonia, lime, magnesia, alumina, and iron have been formed by double decomposition, but their properties would occupy too much space in describing. The mode of obtaining them indicates their composition.

All those salts that contain crystallization water, lose it (efflorescing) when dried over sulphuric acid. This method was employed to determine the quantity of such water present.

Dr. Dalton observed that he had analysed pyroxylic spirit some years since (in 1829), and found it to be composed of an atom of olefiant gas united chemically to one of water. This was inferred from burning its vapours with oxygen in Volta's eudiometer. He also ascertained that burning it in a lamp produced the same heat as burning alcohol diluted so as the two liquids contained the same relative quantities of olefiant gas and water. At the same time he found pyroacetic spirit to be constituted of 3 atoms of carbon, 2 of hydrogen, and 1 of oxygen, or rather 1 atom carbonic oxide holding 2 of olefiant gas: this was discovered by burning the vapour with oxygen in Volta's eudiometer.

On some Combinations of Protochloride of Platina with Protochloride of Tin. By Robert J. Kane, *M.D., M.R.I.A.*

These bodies unite in two different proportions; that containing least tin is of an olive brown colour, crystalline, and very deliquescent; decomposed by much water, giving muriatic acid and mixed oxides of tin and platina. The second, which contains most tin, is of an intensely red colour, soluble in a small quantity of water, giving a splendid red solution, but is decomposed by much water, giving muriatic acid, and a chocolate powder which contains the protochlorides of platina and tin and protoxide of tin. By acting on this powder by ammonia, a black matter in crystalline grains is obtained, which when heated burns like tinder, with formation of peroxide of tin, and platina is reduced.

The colour of the solution was found by Professor Kane, on examination by a prism, to be an absolutely homogeneous red.

Professor Johnston read a paper on the physical cause of certain optical properties observed in chabasie.

The nature and amount of the double refraction are found to vary according to the course taken by the ray; and this Mr. Johnston conceives to arise from the fact of the index for quartz being nega-

tive, and that for chabasie positive, and from certain crystals of chabasie including an excess of silica, which is a substance plesiomorphous with chabasie. (Dr. Thompson gave it as his opinion that the phænomena in question arise from the admixture of two distinct species of chabasie, one of which includes soda and the other lime.)

Professor Johnston stated verbally the results of his analysis of the single and double iodides of gold, results which he found to correspond generally with those already obtained for the chloride.

Professor Graham gave an account of some recent researches which he has published in reference to the constitution of certain compounds as far as respects their constituent water. He illustrated his views by sulphuric acid, with 1 and 2 atoms of water, by oxalic acid with 1 and 3 atoms of water, and by nitric acid containing 1 and 4 atoms of water. Other compounds were also adduced, such as oxalate of magnesia, which contains two atoms of water, or that which may be considered as the water of crystallization of oxalic acid. The oxalate, binoxalate, and quadroxalate of potash, and several other saline compounds were also brought forward in explanation of his views.

Anhyd. oxal. a. . $(\ddot{C}+\dot{C})$

Oxal. water. . . . $(\ddot{C}+\dot{C})\,\dot{H}$

Oxal. acid. . . . . $\dot{H}\,(\ddot{C}+\dot{C})\,2\,\dot{H}$

Oxal. potash. . . $\ddot{K}\,(\ddot{C}+\dot{C})\,\dot{H}$

Binox. pot. . . . . $\dot{K}\,(\ddot{C}+\dot{C})\,\dot{H}+(\ddot{C}+\dot{C})\,2\,\dot{H}^2$

Quadrox. pot. . . $\dot{K}.(\ddot{C}+\dot{C})\dot{H}+(\ddot{C}+\dot{C})\,2\,\dot{H}+\dot{H}(\ddot{C}+\dot{C})\dot{H}^2$

He then drew attention to ammonia, which he considered as frequently performing the function of water in saline compounds; a view which he impressed upon the Section by drawing attention to the composition of the sulphate and of two distinct ammoniurets of copper.

On a new method of testing the presence of Muriatic Acid in Hydrocyanic Acid. By Professor Geoghegan.

This proceeding is essentially preliminary to the adoption of the usual modes of determining the strength of any given specimen of this agent. The insoluble compounds into which the chlorine of muriatic acid enters, and by the formation of which chemists usually recognise its presence, are known to resemble, in many respects, those to which cyanogen gives rise when combining with the same bases. The method proposed by Dr. Geoghegan is founded on the property which the double salt of the iodide of potassium and bicyanide of mercury possesses of being decomposed by acids, and then producing biniodide of mercury. This compound, which has

been analysed by Liebig, and subsequently by Dr. Apjohn *, is easily prepared by mixing, in the proportion of atom and atom, the iodide of potassium and bicyanide of mercury, each dissolved in a small quantity of hot water. After a short time silvery scales (resembling acetate of mercury) are formed, which constitute the salt in question. The circumstance of this salt being decomposed by all the ordinary acids, would appear to show that it is not capable of demonstrating the presence of muriatic acid in particular; but as the only other impurities likely to be present in the hydrocyanic acid are sulphuric and tartaric acids, if the appropriate tests of these latter do not indicate their existence, then the formation of biniodide of mercury on the addition of a crystalline scale, or solution of the double-salt above mentioned, may be considered as furnishing conclusive evidence of the presence of muriatic acid. It may be also stated, that the only hydrocyanic acid likely to contain sulphuric—that prepared from the ferrocyanide of potassium—can be generally recognised, as to the source from whence derived, by its possessing a slight bluish or bluish green tinge, which is quite distinctive. The mode of detecting the presence of muriatic acid above detailed has the advantage over those usually employed, of being very readily applied, and the formation of the reagent is perfectly simple; it is capable of detecting 1-4500th part of the acid: if no change of colour ensue on the addition of the salt, we may conclude that the specimen of hydrocyanic acid contains no impurity which can interfere with the subsequent estimation of its strength. This method, however, is inapplicable to the alcoholized acid of Germany, as the biniodide is soluble in spirit, yielding a colourless solution. If the presence of muriatic acid have been ascertained, its neutralization can be readily effected by the addition of successive small portions of precipitated carbonate of lime, as long as any is dissolved; when free, muriatic acid has been got rid of, and not till then can the estimate of the strength of the specimen under examination be proceeded in with any hope of a correct result. The method of Dr. Ure for effecting this latter end is sufficiently correct for ordinary purposes, if we substitute for the red precipitate which he employs, pure peroxide of mercury; as, independent of the presence of minium and other impurities, red precipitate is seldom, if ever, free from pernitrate of mercury: if perfect accuracy be desirable, the best method, and probably as simple a one as that just alluded to, is the formation of cyanide of silver by the addition of the nitrate of that metal.

On Bleaching certain Varieties of Turf for the Purpose of producing a White Fibre for the manufacture of Paper. By R. Mallet.

The kind of peat used for this purpose is that which exists immediately beneath the vegetable surface of almost every lowland or flat bog in Ireland, and is found existing in a stratum frequently

* [Dr. Apjohn's paper on this subject was published in Phil. Mag. and Annals, N.S. vol. ix. p. 401.—Edit.]

of about three feet thick. It consists of the leaves and stems of various mosses, the roots and fibres of many small aquatic and marsh plants, &c. in the first stage of that very slow decomposition which is the character of every peat moss.

The fibres are tough, and retain perfectly, in most instances, their original form, and are arranged more or less in parallel strata; its colour is a reddish brown, and its specific gravity, as obtained from various bogs, varies from ·360 to ·650. It is proposed either to use the fibre bleached from this for paper-making alone, or in place of the various adulterations now used in paper from rags, such as chalk, gypsum, clay, cotton *flyings*, hair, leather-cuttings, hop-bines, &c.

The same material is capable without bleaching of being converted into an excellent species of board paper or mill-board, by simple pressure under an hydraulic or other press, and subsequent saturation in an exhausted vessel, with glue and molasses, drying oil, rosin, and oil, or any other suitable material. When so treated, it will withstand well the action of high-pressure steam.

This species of turf contains from 3 to 11 per cent. of ashes when humid, and when dried, merely atmospherically, from 4 to 6 per cent. of water. The ashes are of a white or yellowish white colour, and contain,

Carbonate of lime	69·5	
Silica	3·0	
Alumina	17·0	
Peroxide of iron	8·0	
Loss	2·5	100.

The author cannot account for the loss on this analysis, and has been unable to repeat it. He states that ashes from the bottom of the same bog where this red turf was obtained give a totally different result, viz.

Carbonate of lime	21·	
Sulphate of lime	5·5	
Silica	24·5	
Alumina	26·3	
Oxide of iron	22·0	
Loss	0·7	100.

The fibrous matter of this red turf is intimately combined with various complicated vegetable results of slow decomposition, but containing in greatest proportion the extractive matter to which Berzelius has given the name Geine, from γῆ, *terra*. The extract obtained from turf in the way about to be described seems to be nearly the same as that which he describes, in fact to be ulmin in an impure state.

The specimen of turf to be bleached for paper is softened in cold water until its parts by agitation will separate; the finer particles are washed off; the fibre which remains is digested in the cold with a very dilute solution of caustic potass or soda, containing only 50 grains of alkali to a quart of water. The solution, containing the geine in solution, is pressed from the fibres; the latter are then

soaked for some time in very dilute sulphuric acid, consisting of 150 grains of the sulphuric acid of commerce, in a quart of water. The iron is obtained in solution, and the ammonia if any exist in the turf. The fibre is now again separated by pressure from the dilute acid, and digested in the cold, with dilute solution of chloride of lime, of the strength commonly used by paper-makers to bleach fine rags. After the bleaching has taken place the fibre is strained from the liquor, well washed, and applied to the manufacturer's purposes.

The extremely dark-coloured solution obtained by the caustic alkali is now treated with an excess of dilute sulphuric acid, and the acid of the previous washings may be in part used by the manufacturer for this purpose. The alkali is neutralized, and the geine precipitates. It is collected on a filter or by other suitable means, and well washed with cold water, and finally dried by a steam bath, after which, if perfectly dried, it ceases to be soluble in water. It may now be used either in oils or distemper as a colour, being a rich brown bistre.

The solution from which it has been separated contains sulphate of potass, and occasionally, in very minute quantity, sulphate of ammonia.

The quantity of soluble matter in the turf operated on was found from 14 to 30 per cent.; and from one hundred weight of turf of proper quality may be obtained about 18 pounds of fine white fibre fit for paper-making, and a much larger proportion of a coarser and less white description.

When the turf is digested in the chloride of lime, a thin film of an unctuous-looking matter floats after some time on the solution, and by careful management may be obtained in small quantity; it appears to be a mixture of a gum resin with something analogous to wax, and of artificial camphor.

This substance smells like common camphor. Its specific gravity is 0·990, which is a little more than that of camphor. It is at ordinary temperatures always partly solid and partly fluid. When deprived of adhering water it shows a tendency to crystallize; the more fluid part gradually evaporates when it is exposed to air, and a varnish is left on the vessel which contained it. Its point of homogeneous fusion is somewhere between 290 and 300; it evaporates rapidly between that and its boiling-point, which seems to be about 360. As it boils away, its boiling-point rises; it is insoluble in water; a great part dissolves in alcohol, and the remainder is soluble in caustic potass and in fixed oils.

Proof spirit dissolves from it a very minute quantity of a substance which seems to be a gum resin. It is entirely decomposed by a red heat, in close vessels, and also by concentrated and boiling sulphuric acid, which reduces it to charcoal, and a substance apparently analogous to artificial tannin.

The bistre, or colouring-matter, obtained from the turf is not affected by carbonic acid, nor by sulphuretted hydrogen, nor by protochloride of tin: strong nitric acid will not change its colour, although

by long standing it is decomposed by it. Chlorine bleaches it slowly; caustic alkalies redissolve it. It is scarcely bleached at all by the sun's rays, nor does it when properly washed and dried show any tendency to deliquesce; it therefore is an excellent colour for paper-staining and other such purposes, as few common agents will injure it, and it can be readily removed from surfaces by an alkali*.

The proportions of useful products above given can only be considered as approximations, having been deduced from experiments on a small scale; they would probably be much increased, and the relative expense of preparing the material reduced, if the process were carried on with greater quantities.

On some singular Phænomena of Flame from Coal-Gas. By R. Mallet.

If an Argand gas-burner be lighted, and a conical tube of a certain diameter be inserted concentrically within it, with its extremity entering a certain distance, within the burner, and, while the gas is inflamed, a current of air be propelled through the conical tube in the same direction with the streams of gas, under certain conditions, the whole of the gas-flame will retract or be drawn back between the internal surface of the burner and the external surface of the conical tube, and nothing whatever will pass forward but a stream of strongly heated carbonic acid and aqueous vapour. This very singular phænomenon of the passage in opposite directions of two currents in such close contact does not appear to be affected by the size of the burner, provided a certain proportion be preserved between it and the conical air-tube. The experiments were made with two burners chiefly, one of which was three quarters of an inch internal diameter and one and a half inch deep, measured along its axis, and the other seven sixteenths of an inch internal diameter, and one and three eighths inch deep.

With these it was found that the retraction of the flame was produced most perfectly in the case of the large burner by a tube of five sixteenths of an inch diameter, but yet took place to a certain extent until the diameter of the tube was reduced to one eighth of an inch, and in the case of the smaller burner it was most perfectly produced by an air-tube of three sixteenths of an inch diameter; yet taking place in a slight degree with one of only one twentieth of an inch diameter.

If the conical air-tube be not inserted into the burner, but merely held close to its base or lower aperture, no retraction takes place, the flame is merely curtailed, and the combustion rendered more perfect; and the same result takes place when a tube equal in diameter to the internal part of the burner is used, in which case it is obvious none of the flame could retract.

To the perfect production of the foregoing effects it is necessary that the apertures for the gas in the burners be of a much smaller

* [See a paper by the late Dr. Macculloch, in Trans. Geol. Soc., first series, vol. ii.; or Phil. Mag., first series, vol. xliv. pp. 215, 271.—Edit.]

size and more numerous than usual. When the axis of the conical air-tube is parallel with that of the burner, the direction of each separate jet of flame from the holes in the burner is also parallel to the same while the air-tube and burner are respectively concentric; but if, while they remain concentric, the axis of the air-tube be inclined to that of the burner, a far more singular effect ensues: each separate jet of flame now in retracting describes a spiral round the internal surface of the burner, making from one third to perhaps one half a revolution.

If the conical air-tube, while still inclined as above, be now brought into contact with that side of the burner towards which it is inclined, the obliquity of the spiral is much lessened; but the flame is so much retracted at the side of the burner opposite the air-tube that it makes its appearance out at the lower end of the burner. The same effects are produced whether the burners are vertically, up or down, or horizontal, or inclined at various angles, subject to merely the disturbances produced by the ascent of the neighbouring currents of heated air.

The effects do not seem to depend upon difference of temperature between the current of air and the flame, as no change is produced by heating the former to upwards of 600° Fahrenheit, neither does the angle of the cone seem to be very essential, except it be so great as to nearly stop the aperture of the burner. A cylindrical tube answers equally well with a cone, but an inverted cone, that is, a tube terminating with an enlargement, will not produce the effects. Tubes of various other forms produce corresponding variations of the principal phænomena. A large flat disc, with an aperture just large enough to admit the burner, placed close to its perforated extremity, so as to prevent the passage of external currents parallel to the internal current of air, does not change the effects.

The retraction is considerably lessened, however, by stopping up the space at the lower end of the burner, between it and the air-tube, but is not wholly destroyed.

Another singular fact connected with these remains to be mentioned: if a glass or copper tube, of about three eighths of an inch greater diameter than that of the burner externally, be placed over it, the same sonorous effect is produced as in the well-known experiment of the combustion of pure hydrogen, but much louder; indeed, the copper tube used, which was eighteen inches long and one and three eighths inches diameter, emitted a most overpowering sound. Length of tube produced no variation in the state of the flame, nor did increase of diameter over the above limits, although both produced of course a change of musical note; but if the diameter of the tube, whether of glass or copper, was reduced to very nearly that of the external diameter of the burner, on approaching the end of the tube with the burner, the retractile flame was drawn forward, and, unless skilfully managed, was drawn out or extinguished at the moment the burner entered the tube; if, however, the introduction was successfully effected, the moment the

burner came within the tube the flame again retracted as before. The sound ceased at the moment that the flame was extinguished.

The pressure of gas used in most of the experiments was that of the ordinary main-pipes in this city, about $1\frac{3}{4}$ inch of water; that of the current of air, which was produced by a good pair of double bellows, was equal to the pressure of a column of ($2\frac{1}{2}$ inches of) mercury; but it was found that no material alteration of effect took place from condensing the gas to about two atmospheres, and causing it to issue inflamed at that pressure, provided the pressure of the current of air was likewise increased in the same ratio nearly.

With a less powerful stream of air than was above stated, the effects were imperfectly produced; and with a much more powerful one the flame was blown out.

The temperature of the current of air heated by the flame, when it retracted best, was found, at the distance of four inches from the burner, to be 432° Fahrenheit, or perhaps a little higher. The combustion of the flame in all the foregoing cases is absolutely perfect; its colour is a deep blue, and the volume of intensely heated air propelled is very great, so that it may be rendered very useful for various purposes in the laboratory. It is not perfectly dry, but it is free from dust or smoke.

On the Volatilization of Magnesia by Heat. By Professor Daubeny.

According to Von Buch, carbonate of magnesia must have been sublimed by volcanic action, although such a phænomenon would, Dr. Daubeny conceived, be scarcely admitted by chemists as consistent with the known properties of that earth.

A curious fact, however, confirmatory of the truth of Von Buch's opinion, occurred to Professor Daubeny in Italy. He visited a locality where there was an upper stratum of lava containing cavities. In one of these an English gentleman, resident on the spot, discovered a large quantity of carbonate of magnesia, and Professor Daubeny himself observed a minute portion of the same earth coating the outer surface of the lava. Here it is difficult to understand in what manner this substance could collect in the cavities or upon the surface of the rock, unless it had previously become volatilized by heat*.

(Dr. Dalton observed that there could be no doubt that carbonate of magnesia might be volatilized, since Dr. Henry had informed him that a quantity of this substance was always driven off whenever the heat was carried beyond a certain point.)

Mr. Hartop made a communication on the use of the hot air blast in the manufacture of pig iron, in which he showed that the saving said to be effected by the use of hot air had been overrated, as a considerable portion of the alleged saving had been previously effected by other improved processes.

* [Other particulars relative to this subject are given in our last Number, p. 316.—Edit.]

The general saving on the average he stated to be no more than 10*s*. per ton, and observed, that the price of *such* iron in the market had actually fallen from 15*s*. to 20*s*. per ton, while that from cold air at the same time rose 5*s*. per ton in Yorkshire. (This statement gave rise to observations on the part of several gentlemen, who dissented from Mr. Hartop, and stated that the reduction in price of iron from hot air had not occurred in other parts of the country, and that, as prepared in Glasgow and many other places, it had not been so deteriorated. It has in consequence been adopted in every smelting-house in Scotland, and the annual produce of the works in that country during the last ten years has been nearly doubled.

Reference was also made to processes adopted in the Russian smelting-works, which showed that by a judicious adjustment of the quantities of cold air introduced by the blast, a saving could be effected approaching even to that obtained by the use of hot air.)

Account of some Chemical Processes. *By* FRANCIS BARKER, *M.D.*, *Prof. Chem. Trin. Coll. Dublin.*

It has been known since the time of Bergman, that diluted acetic acid has little or no action on peroxide of iron; but it is not, perhaps, generally known that this oxide may be completely separated from sulphuric or muriatic acid, and probably from most other acids, by an alkaline acetate, the alkali exerting its usual action of detaching the peroxide, whilst the acetic acid remains inactive and does not unite with it, and that by means of the acetate of potash, peroxide of iron may be completely detached from the oxide of manganese, one portion of the acetate of potash decompounded by the salt of manganese producing acetate of manganese, which remains in a state of solution, whilst the other portion of the acetate of potash separates the peroxide of iron, on which the diluted acetic acid has no action. The advantages arising from this mode of operating are obvious, as it gives the chemical analyst the means of separating the oxides of iron and manganese by agents easily obtained and in the hands of every chemist.

As the success of this method depends in a great measure on attention to minute details in the mode of conducting the process, more especially on the comparative quantities of the substances employed, a few experiments are adduced.

Experiment.—Five grains of green sulphate of iron taken and dissolved in fifty measured grains of cold distilled water: to this added, from the end of a dropping-tube, six drops of diluted nitric acid, spec. grav. 1·280. On applying heat to this mixture, it acquires a dark olive colour, arising from decomposition of the nitric acid by the protoxide of iron and absorption of the nitric oxide by the ferruginous solution. When the mixture is heated to ebullition this colour disappears, and is succeeded by the ordinary yellow colour of a solution of peroxide of iron. To the solution of the sulphate of iron, thus altered by the action of nitric acid, an aqueous

solution of acetate of potash, containing one tenth of its weight of the acetate, is to be added, in the quantity of two hundred grains measured. The mixture, on this addition being made, changes to a dark reddish brown colour, nearly as intense as that of port wine. The mixture is now to be diluted with its own volume of water, and heat applied until it boils; the ebullition continued for about two minutes. The peroxide of iron begins to separate as the heat approaches the boiling-point, and in a short time the whole peroxide is detached. On filtering the mixture whilst hot, the fluid which passes through the filter appears colourless, and on addition of the the triple prussiate of potash, affords neither precipitate nor blue tinge indicating the presence of iron. The powder remaining on the filter, well washed with hot water, is of a clove brown colour.

The addition of the nitric acid with subsequent ebullition is essentially requisite to the success of this experiment; for if the green sulphate of iron be employed without the addition of nitric acid, on adding the solution of acetate of potash, and causing the mixture to boil, no change of colour to reddish brown is found to take place, but a black powder separates, and the mixture when filtered affords a fluid of a strongly ferruginous taste, yielding an abundant precipitate, of a bluish white colour, with the triple prussiate of potash; thus proving that the conversion of the oxide of iron into peroxide must precede the addition of the acetate of potash, which is otherwise incapable of separating the oxide of iron from the acid.

When a solution of the green muriate of iron is treated in a manner similar to that above described, by converting the protoxide of iron into peroxide by nitric acid, and decompounding the solution by acetate of potash and heat, the same effects are produced as in the green sulphate of iron.

If to a solution of peroxide of iron, produced by the method above described, a solution of the oxide of manganese is added, then solution of acetate of potash and heat applied, a similar deposition of peroxide of iron takes place; and the filtered liquor, on addition of triple prussiate of potash, affords a cream-coloured deposit unmixed with any blue tinge: the peroxide of iron has, therefore, remained on the filter, and the oxide of manganese in solution has passed through, yielding its proper precipitate with the triple prussiate. It is right to observe, that a solution of the muriate of manganese is not rendered turbid by admixture with acetate of potash and subsequent application of heat.

It follows from the preceding experiments, which have been many times repeated, that peroxide of iron may be completely separated from either sulphuric or muriatic acid by acetate of potash, and that in a mixed solution of peroxide of iron and oxide of manganese in an acid, a complete separation of the peroxide of iron may be effected by means of the acetate, provided that proper attention has been given to the comparative quantities of the ingredients employed in the mixture. Acetate of soda or of ammonia may be substituted for acetate of potash in producing this decomposition.

Two other chemical facts were adduced.

1. As the precipitation of the ammoniacal phosphate of magnesia is accelerated and made manifest by drawing lines with a blunt glass rod on the internal sides of the glass vessel in which the proper mixture is made for producing the precipitate, a fact first noticed by the late Dr. Wollaston, so in a similar manner the separation of bitartrate of potash from any mixture containing potash, to which tartaric acid has been added in proper quantity, will be accelerated and rendered manifest by drawing lines with pressure on the internal sides of the vessel with a glass rod, the crystals of bitartrate first attaching themselves to these lines.

2. That nitrate of lead like the nitrate of baryta is precipitated from water by addition of strong nitric acid, which in each case exerts a similar action, namely, that of abstracting the water from the salt.

On a Source of Inaccuracy in Observations of the Dew-point. By the Rev. WM. VERNON HARCOURT, *F.R.S.*

Mr. Harcourt having observed an apparent variableness in the deposition of dew on different surfaces, at the same temperature and in the same atmosphere, was led to make the following experiments.

A pane of glass was rubbed, on different portions of its surface, with substances of different degrees of hardness, and left till the equality of temperature was restored: being then breathed upon, it was observed to show the condensed vapour in proportion to the polishing power of the substances by which the different parts of the glass had been rubbed; characters traced by a leaden point displayed this phænomenon in the greatest perfection. The experiment was next tried on metallic surfaces, by polishing, for instance, part of the blade of a rough razor, and breathing on it, when the same effect was obvious.

When the state of the dew on the different surfaces was examined with a lens, it appeared that its greater visibility on the more polished parts was owing to a stronger reflection of light from a greater number of minute and unconnected drops deposited on those parts. It would seem as if the process of polishing insulates the points to which the particles of vapour attach themselves, and prevents them from running into each other; but though the vapour condensed on the polished surface thus becomes more sensible, it is not increased in quantity, as is easily proved by continuing to breathe on the pane of a window till streams of water run down on the unrubbed surface, and on that only; and it is not a little remarkable, if the polish be also carried horizontally along the lower part of the pane, to observe the streams dammed up where they meet the polished part, and drops of water left along that portion of the line.

The observation of these facts led the author to apply some practical corrections to the ordinary method of ascertaining the dew-point: he adopts the direct process of Dr. Dalton, reducing the

scale of the operation, and substituting metal for glass. A highly polished metallic vessel, not more than ⅘ths of an inch wide and 1¼ inch long, is nearly filled with water; some crushed sal ammoniac is introduced; the salt is stirred up and mixed with the water by the bulb of a small thermometer, which falls in consequence very gradually, and when the dew appears the thermometer is in contact with the surface on which it is deposited. If a considerable depression of temperature is required, the vessel may be cooled down previous to the experiment by a similar process. This instrument, from the small quantity and cheapness of the cooling material, may be used constantly without extravagance, and from the conducting and radiating properties of the vessel, as well as the precision with which it indicates the first deposition of dew, may probably be found to be uniform in its results.

Mr. Moore laid before the Section a leaden pipe which had served for about twenty years as the worm of a still, for the distillation of medicated waters and spirits; at length it began to leak, and on examination it was found to be supported at various points by bars of wood crossing it, and to be tied at others with twine. Wherever it thus came in contact with either wood or twine, it was deeply corroded, and the lead appeared to be converted into a dark powder, which, when examined, was found to contain oxide and chloride of lead; at all other points the pipe was perfectly sound. The appearance of the corroded parts was sufficiently against its being attributed to mechanical action. The presence of chloride and oxide in the powder, established, he thought, that the corrosion was not entirely, if at all, caused by acids formed by the decaying organic matter; it appeared to him that it ought rather to be attributed to galvanic action, developed by the contact of the metal, and wood, or twine, which cause, acting for such a length of time, might be sufficient to accomplish the destruction of the pipe at the points of contact.

Notice of certain Appearances in Coagulated Blood. By Dr. Newbigging.

The author has observed on the under surface of a clot of blood, received into a cup variously coloured within, certain bright red spots, corresponding accurately to the figure of those parts of the interior of the cup which were coloured green, while in all other parts the coagulum was of a dark tint. Experiments were tried on various sorts of china, with different colours and glazing, but always with the same result. The contrast of the brightness of these red spots with the darkness of the general mass lasted nearly ten minutes after exposure to the air.

On a Modification of the Safety Lamp. By W. Ettrick.

The author, referring most of the unfortunate explosions which

have happened in collieries where the "Davy lamp" is used, to the ignorance and wilfulness of the workmen, proposes, for the security of the lamp from injury and mismanagement, that the body of it should be formed of a strong spherical glass, furnished with a copper tube above and below, and that surfaces of wire gauze should be placed at these points, protected by parallel metallic plates, so as to be inaccessible to the workmen. The glass is defended externally by strong arched wires. It is also provided that the upper and lower parts of the lamp should be connected by means of a spring, so as to permit of a *sliding motion*, for the purpose of deadening in one direction the effect of any shock or concussion. According to this arrangement the air can only enter below the flame and pass up within and through it, and the products of combustion are conducted directly upwards to a proper aperture in the cover of the lamp.

Professor Graham noticed with regard to safety lamps, on the theory of which he has been some time engaged, that wire gauze is rendered much more impervious to flame by being first dipped in an alkaline solution, which also protects the wire from oxidation.

On a new Electrometer. By William Snow Harris, *F.R.S., &c.*

Report of the Committee appointed to consider the subject of Chemical Symbols. By Dr. Turner.

[This Report, with remarks of several of the members of the Committee, will appear in the next volume of the Transactions of the Association.]

ROYAL SOCIETY.

June 4.—Professor Faraday's Tenth Series of "Experimental Researches in Electricity," was read.

This paper relates altogether to the practical construction and use of the voltaic battery. Guided by the principles developed in former series, the author concluded that in voltaic instruments in which the copper surrounded the zinc, there was no occasion for insulation of the contiguous coppers, provided they did not come into metallic contact; and therefore in the construction of some new instruments he interposed paper only between the coppers instead of the usual insulating plate of porcelain or glass. The battery thus constructed is essentially the same with Dr. Hare's; and the author recommends even his form of trough for the purpose of putting the acid on to, and moving it from the plates. By attending to certain points described, as many as 40 pairs of plates could be packed into a space not more than 15 inches in length, and thus a very portable, and, at the same time, powerful and convenient trough might be obtained.

In comparing this form of trough with others, the author used acids of constant strength, took note of their quantity, allowed them to act in the troughs until the power of the apparatus had nearly ceased,

estimated the quantity of effect by his volta-electrometer, and then estimated the quantity of zinc in the battery employed in producing the effect by the results of an analysis of a given portion of the exhausted charge. In this way it was easy to tell how much zinc was dissolved from any one plate, or from all the plates, and to compare it with the quantity of water decomposed in the volta-electrometer. Thus, with a perfect battery of 40 pairs of plates, an equivalent of water decomposed in the volta-electrometer would be the result of the solution of an equivalent of zinc from each zinc plate, or forty equivalents in the whole; but with a battery not so perfect, a greater proportion of zinc would be dissolved by the acid in the cells.

When the new battery was thus compared with that of the ordinary form, it was found to have greatly the advantage. Thus, with 40 pairs of plates, the former lost 2·21 equivalents at each plate, and the latter 3·54. With 20 pairs of plates, the former lost 3·7 per plate, and the latter 5·5. With 10 pairs of plates, the former lost 6·76 per plate, and the latter 15·5. The author refers to two difficulties still existing in the construction of the battery, but considers its value so great as to deserve receiving that degree of attention by the application of which these difficulties may be removed.

The author then investigated many other practical points in the use of the battery, ascertaining the influence of various circumstances in the manner already described. Thus he found nitric acid to give a higher result of voltaic action than sulphuric or muriatic acid; the quantity of zinc dissolved in order to produce decomposition of an equivalent of water being only 1·85 per plate when nitric acid was used, 3·8 when muriatic acid was used, and 4·66 when sulphuric acid was employed. The acid which he afterwards used as the best for ordinary purposes consisted of 200 water, 4·5 oil of vitriol, and 4 nitric acid.

The mode of proof adopted by the author was of course independent of the strength of the acid; as was shown by making experiments with the same acid at very different strengths; thus, when nitric acid was used, and the strengths were as 1, 2, and 4, the proportion of zinc dissolved was very nearly the same for the water decomposed. The same result was obtained when sulphuric acid was employed.

The different circumstances of uniformity of charge—purity of zinc—foulness of the zinc plates—new and old plates—vicinity of the copper and zinc—doubling of the copper—first immersion of the plates—number of plates—size of the plates and simultaneous decompositions—were then considered, and such of them as would admit of experimental comparison in the manner already described were put to this test.

GEOLOGICAL SOCIETY.

May 13th.—A paper was first read, "On the Cretaceous and Tertiary Strata of the Danish Islands of Seeland and Möen;" by Charles Lyell, Esq., P.G.S.

The author commences with a short account of the succession of

deposits formerly supposed by Dr. Forchhammer to exist in the Danish islands. In a memoir published in the "Edinburgh Philosophical Journal of Science" for July 1828, Dr. Forchhammer had described the white chalk of Seeland as covered by a coralline limestone, and had imagined the white chalk of Möen to be a formation higher in the series than this coralline limestone. He had also considered certain deposits of blue clay, sand, and gravel, seen in the cliffs of Möen, as alternating with the white chalk.

Mr. Lyell examined, in company with Dr. Forchhammer, the cliffs of Seeland and Möen, during the summer of 1834, and the following are the results at which he has arrived. The two formations of which Denmark and Danish Hölstein chiefly consist, are, chalk, and an overlying tertiary deposit. Part of the latter resembles in composition the argillaceous and sandy beds of the English crag. Another part corresponds with deposits usually called diluvial, especially those associated with the English crag, in parts of Norfolk. Large erratic blocks are also strewed over the surface of Denmark, connected with, and sometimes buried in, the gravel or "diluvium". In some sections on the banks of the Elbe, the yellow tertiary sands are divided regularly into thin strata, and are exposed for a thickness of about 200 feet. Unstratified masses of blue clay, of great thickness, are also there seen, in which gravel, containing every kind of rock, from granite to chalk, occurs.

There is often an abrupt passage from the stratified to the unstratified parts of the formation, which the author compares to the Norfolk and Suffolk crag, from its general appearance, but without pretending to decide its relative age in the tertiary series. Fossils are rare, except those washed out of older strata. A few recent shells have been found near Segeberg, and at other points, and two of extinct species at Schulan on the Elbe.

The white chalk of Denmark is characterized by the same fossils as those of the upper chalk of France and England, and occurs at Stevensklint in Seeland, and in the cliffs of Möen. On the coast at Stevensklint, and at several places in the interior of the same island, particularly at Faxoe, a newer limestone occurs, consisting, for the most part, of fragments of coral, cemented together by a chalky matrix. It is separated from the white chalk by a thin seam of bituminous clay, containing marine shells and impressions of plants. The limestone contains beds of flint, like those of the white chalk, but more cherty, and usually in continuous layers; and these are sometimes disposed diagonally to the general lines of stratification. The author presumes that this coral limestone, which he calls the Faxoe limestone, may be the equivalent of the Maestricht beds, as, like them, it contains some fossils identical with those of the chalk, intermixed with others which belong to genera more usually characteristic of tertiary formations*.

* [As Dr. Forchhammer states that in Seeland this coralline limestone reposes upon a formation which is the equivalent of the *calcaire grossier*, it seems probable that it may itself be the equivalent of the lower or coralline crag of Mr. Charlesworth; especially as it would appear, from a comparison of the details given by Dr. Forchhammer with the above notice of

The shells at Faxoe are chiefly in the state of casts, and among them are several species of Cypræa, Conus, Mitra, and Voluta, as also an Ammonite, a Patella, a Fusus, and a Cerithium. Upon the whole, there are in the collection of Prince Christian of Denmark 132 species of fossil shells from the Faxoe beds, of which 26 have been ascertained by Dr. Beck to be identical with chalk species, while the rest are distinct from them, but do not agree with known tertiary shells.

Lastly, the author considers the relations of the chalk of Möen with the tertiary strata of that island. The white cliffs of Möen are from 300 to 400 feet in height, consisting of chalk and parallel layers of nodular flint, the strata having been violently disturbed; so that instead of being nearly horizontal, as at Stevensklint, they are curved, and often vertical, and, upon the whole, more deranged than the chalk of Purbeck and the Isle of Wight.

The range of lofty cliffs in Möen is divided into separate masses by ravines, which often intersect them from top to bottom, but are in great part filled up with tertiary clay and sand, masses of which appear to have subsided bodily into large fissures and chasms of the fractured chalk. In consequence of these disturbances the chalk has been made to alternate on a great scale with interposed and unconformable strata of clay and sand. These alternations cannot be explained by supposing the detritus of the superincumbent strata to have been washed in by running water into clefts; but masses of the tertiary beds seem rather to have been engulfed. Several drawings illustrating these dislocations accompany the memoir, and the appearances are compared to those exhibited by the chalk, nearly enveloped by crag, near Trimingham in Norfolk, although the entanglement of the two formations there, is on a smaller scale.

Dr. Forchhammer now agrees with the author in the principal conclusions above enumerated, and has discovered the disturbed chalk of Möen in the South of Seeland, as also the Faxoe beds overlying chalk in Mors, an island of the Lym Fiord.

A paper was afterwards read, "On a peculiarity of Structure in the Neck of Ichthyosauri, not hitherto noticed;" by Sir Philip Grey Egerton, Bart., M.P., V.P.G.S.

Miss Anning of Lyme Regis discovered, a short time since, in a thin bed of lias shale, near that town, a large portion of the skeleton of a new gigantic species of Ichthyosaurus. Among these interesting remains are the anterior cervical vertebræ, together with an occipital bone, and it is to the peculiarity of structure which they present that Sir Philip Egerton principally confines his observations. The occipital bone, he says, on the authority of Mr. Owen, proves very satisfactorily the permanent separation of the basilar element of the occiput

Mr. Lyell's paper, that the "Faxoe limestone" occupies an inferior position in the series to the tertiary beds which Mr. Lyell compares to the "argillaceous and sandy beds of the English crag," termed by Mr. Charlesworth the upper or *red crag*. Mr. Lyell's paper was read, before Mr. Charlesworth's researches on the crag had been communicated to the Geological Society. See Lond. and Edinb. Phil. Mag. for August, p. 81.—E. W. B.]

in individuals of the fullest growth and largest size, evincing a very languid circulation in this family of reptiles. The atlas and axis of Ichthyosauri, the author states, are usually found adhering together, the connexion between them being so intimate that it is rarely possible to disunite them; and when this has been effected, the surfaces have borne the appearance of fracture more frequently than that of natural division. In one instance in which he succeeded in separating the two bones, the articulating surfaces were nearly even, and without cup. This union of the two vertebræ appears to have received additional strength from a small bone which articulated with the under circumference of the atlas and axis, showing, as the author observes, that in the anterior region of the spinal column strength and not latitude of motion was required. This bone is a nearly circular, solid, umbonated disc; the central projection being on the inferior or external surface, while the upper is depressed anteriorly and posteriorly for the purpose of articulating with the atlas and axis, the two surfaces being divided by a transverse elevation corresponding with the line of union of the vertebræ. The atlas and axis have their circumferences prolonged in the form of two tangents meeting at an obtuse angle on the under surface. These processes are truncated, and form, when the vertebræ are in apposition, a triangular depression for the reception of the two articulating surfaces of the interspinous bone. Sir Philip Egerton states that Mr. Owen has informed him, that a bone somewhat analogous in position, although not in form, occurs in some recent saurians. The apparently two succeeding vertebræ present, at the lower part of their articulating surfaces, an alternating elevation and depression, fitting into each other so exactly, as to limit, to a great extent, the motion between the bones. Some of the other cervical vertebræ are also remarkable for the flatness of their surfaces, the intervertebral cavities being nearly obliterated. In conclusion, the author says, that the conditions under which the atlas and axis are found; the existence of an auxiliary bone connecting the two; the form of the articulating surfaces of the cervical vertebræ, and the consequent contraction of the intervertebral cavities, all tend to prove that the extent of motion in the cervical region of Ichthyosauri was extremely limited, at the same time that its strength was proportionally increased.

May 27th.—A paper was first read, "On certain Lines of Elevation and Dislocation of the New Red Sandstone of North Salop and Staffordshire, with an account of Trap Dykes in that Formation, at Acton Reynolds, near Shrewsbury;" by Roderick Impey Murchison, Esq., V.P.G.S.

The author refers to former memoirs, read before the Society, in which he pointed out the existence of certain bedded trap rocks, interstratified with transition deposits, and of other intrusive trap rocks which have been subsequently injected amid these stratified masses. The Breiddin Hills, west of Shrewsbury, afford examples of both these classes of trap rock, in ridges running from west-south-west to east-north-east, and also indicate, upon their flanks, that elevations have taken place along these lines, subsequently to the deposition of the

adjoining coal measures. The author has lately discovered that still more recent movements of elevation have been propagated along the same line of fissure, posterior to the consolidation of the new red sandstone. He was led to this observation by the unexpected discovery of three small trap dykes beneath the house of Sir A. Corbet, Bart., at Acton Reynolds, which were accidentally laid open upon clearing out the foundations of that mansion.

These dykes cut like walls through the new red sandstone, and are made up of a peculiar greenstone and a mottled concretionary felspar rock, both of which rocks occur in the Breiddin Hills. Besides this similarity in structure, the principal dyke has precisely the same direction as the Breiddin Hills, and hence the author was induced to examine the intervening tract of fifteen miles by which these trap rocks are separated. The result has been the detection of an anticlinal line throughout that space, along which the strata of the new red sandstone are thrown off, both to the south-south-west and north-north-east, or at right angles to the line of eruption of the trap. The clearest and most unequivocal point in the course of this anticlinal line is seen in Pim Hill, six miles north of Shrewsbury, in the centre of which the sandstone is compact, white, and unstratified, with slickensides, coatings of earthy oxide of manganese, traces of copper ores, vertical fissures, &c., whilst strata of unaltered sandstone dip away from this common centre, both to the south-south-west and north-north-east. This point of altered rock lies exactly upon the line connecting the Moel y Golfa ridge of the Breiddins with the trap dyke of Acton Reynolds. The line of elevation is further traceable for about fifteen miles, to the east-north-east of Acton Reynolds, usually throwing the strata into only dome-shaped masses; but the Rev. T. Egerton has observed it passing the Liverpool and Birmingham canal thirty miles distant from the Breiddin Hills.

The author is of opinion that the hilly range of new red sandstone extending from the Ness Cliff Hills, by the south of Wem, into the Hawkstone and Hodnet Hills, and then prolonged by the south of Market Drayton into the high grounds of Ashley Heath (parallel to the line extending from Moel y Golfa through Acton Reynolds), has been affected by similar elevatory forces acting along a line proceeding from the focus of the principal ridge of the Breiddins, or that on which Rodney's Pillar stands; and in corroboration of this, he alludes to the veined and metalliferous character of the red sandstone along this line, in which copper ores, manganese, &c. are of partial occurrence. Immense accumulations of coarse gravel and clay obscure the flanks, and sometimes hide, for vast spaces, the disturbed and denuded strata of red sandstone along the chief anticlinal line.

Attention is then directed to the position of the lower strata of the new red sandstone, around the coal-fields of Colebrook Dale and South Staffordshire; and in confirmation of opinions expressed in former communications, the author cites several examples near Wolverhampton and Dudley, particularly one at Sedgely, where the coal itself is thrown up at an angle of about 40°, the strike being north and south, with the lower new red sandstone conformable to it; and

from these evidences he concludes that the principal lines of fracture along the margin of these coal-fields took place after the deposition of the new red sandstone series, and that, therefore, the break so prevalent in the South-west of England, between the upper part of the coal measures and the new red sandstone, can no longer be considered as of general application in English geology.

From the amount of dislocation which has taken place throughout all this region, accompanied by an enormous destruction of masses of new red sandstone, and from the protrusion of so many points of trap rock, some of which cut through that formation, the author is disposed to think that the recently described outlier of Lias at Cloverly, and Prees * in Shropshire, *may* have been originally connected with the chief escarpment of lias in Warwickshire and Worcestershire, there being in the large accumulations of gravel in the intermediate country, many lias shells, which may have been derived from the destruction of once continuous strata of that formation.

In conclusion, he recapitulates what in former memoirs read to the Society he has endeavoured to show—

1st, That certain trap rocks have been evolved during the formation of the transition rocks:

2ndly, That others have burst forth subsequently to the consolidation of these older strata, throwing them into vertical and broken forms, and producing metalliferous veins in them:

3rdly, That this period of activity was anterior to the formation of the coal measures, as is proved by the strata of the latter resting unconformably upon the highly inclined edges of the transition rocks.

Carrying on the inquiry from this point, the present memoir demonstrates, 4thly, that igneous agency evolving precisely similar products has been renewed at a much later period upon one of these lines of ancient eruption; and, finally, that the great disruptions around the flanks of the central coal-fields of England took place after the accumulation of the new red sandstone.

A paper was afterwards read, "On the Crag of part of Essex and Suffolk;" by Edward Charlesworth, Esq.; communicated by Edward William Brayley, Esq., F.G.S. This paper has since been published, in our number for August, p. 81.

ZOOLOGICAL SOCIETY.

May 12.—A letter was read, addressed to the Secretary by P. L. Strachan, Esq., and dated Sierra Leone, February 22, 1835. It referred to some *Alligators* sent from that country by the writer several months since, all of which died on their passage. It also stated that he had forwarded to the Society a *Mud Turtle* (*Trionyx?*), which, he hoped, would prove acceptable.

A letter was read, addressed to the Secretary by A. MacLeay, Esq., Colonial Secretary, New South Wales, dated Sydney, October

* [See Lond. and Edinb. Phil. Mag., vol. vi. p. 314, and for July, present volume, p. 59.—Edit.]

25, 1834. It stated that the writer had, in consequence of the application made to him, set on foot inquiries respecting that interesting *Bird* of New Zealand, the *Apteryx Australis*, Shaw; and that he had succeeded in obtaining a skin of it, (destitute, however, of the legs,) which he had forwarded to the Society. The specimen was exhibited.

The skin presented by Mr. MacLeay to the Society was obtained by him from the Rev. W. Yate, who writes to him as follows, dated Waimate, March 10, 1834: "About six weeks ago I had one of these birds in my possession, the second I have seen in the Land. I kept it nearly a fortnight, and in my absence it died. One of my boys took off the skin; the legs rotted off. I have very great pleasure in sending you the skin as it is. Should I ever meet with another, I will do all I can to preserve it for you. Its food is long earth-worms. It strikes with its bill on the ground, and seems to know by the sound where its prey lies. It then thrusts its bill into the ground, draws up the worm, and swallows it whole and alive. They kick very hard, and their legs are remarkably strong for the size of the bird. They are very rare in New Zealand, but are found in the greatest numbers at Hiku Rangi, the mountain which you mention."

Mr. MacLeay adds, that he has applied to other friends on the subject, and that, should he succeed in procuring further information, he will communicate it to the Society.

He concludes by expressing his intention of forwarding to the Society the white-fleshed Pigeon of the Colony, which, he conceives, would be a great acquisition in England: it is certainly, he says, far superior to Partridge.

Colonel Sykes, in illustration of the extended geographical distribution of some species of *Birds*, called the attention of the Meeting to a collection of *Bird-skins*, formed at the Cape of Good Hope by Captain Spiller, R.A., and presented by that gentleman to the Society. The principal object had in view was the demonstration of the identity of many species of *Birds* existing in Southern Africa, with those which Colonel Sykes had himself obtained in Dukhun. By the juxtaposition of the Cape *Birds*, and of those killed by himself in India, he showed that the following species exist equally in both those countries: several of them are also common to Europe.

Falco Tinnunculus, Linn.—South Africa, India, and Europe.
Milvus Govinda, Sykes.—South Africa and India.
Strix Javanica, Horsf.—*Strix flammea*, Linn.? Universal?
Alcedo rudis, Linn.—South Africa and India.
Oriolus melanocephalus, Linn.—South Africa and India.
Coracias Indica, Linn.—South Africa and India.
Upupa minor, Shaw.—South Africa and India.
Cinnyris Mahrattensis, Cuv.—South Africa and India.
Ardea Caboga, Penn.—South Africa, India, and Europe.
Nycticorax Europæus, Steph.—South Africa, India, and Europe.
Limosa Glottoides.—South Africa and India.

Gallinago media, Ray.—South Africa, India, and Europe.
Rhynchæa Capensis, Steph.—South Africa and India.
Cursorius Asiaticus, Lath.—South Africa and India.
Himantopus melanopterus, Horsf.—Universal.

Colonel Sykes remarked that he had previously, while illustrating his 'Catalogue of the Birds of Dukhun', read before the Committee of Science and Correspondence in 1832, shown the specific identity of many European and Indian *Birds*, especially in the orders *Grallatores* and *Natatores*.

"Some account of a hybrid *Bird* between the cock *Pheasant*, *Phasianus Colchicus*, Linn., and the *grey Hen*, *Tetrao Tetrix*, Ej., by Thomas C. Eyton, Esq.," was read. It was illustrated by the exhibition of the preserved skin of the bird, and also of a drawing made from it. It is inserted in No. xxix. of the 'Proceedings' of the Society.

Mr. Gray exhibited, from his own collection, specimens of a *Coral*, known to some of the English residents at Canton by the name of the *Glass Plant.* He stated that it appeared to him to be most nearly related to *Gorgonia*, although it differed widely from that genus by its axis consisting, not of a single calcareous stem, but of a congeries of almost innumerable siliceous filaments, slightly twisted together into the form of a rope. Each of these filaments, however, is composed, like the stem of *Gorgonia*, of very numerous concentric *laminæ*, which are easily separated from each other by exposure to heat, such as the flame of a candle, when the fibre splits down one side, leaving the inner *laminæ* exposed. Near their upper extremity the filaments have a wrinkled appearance, and are furnished with numerous barbs, directed backwards; towards the base they taper gradually, and become much attenuated. The crust bearing the polypes surrounds the mass of siliceous filaments, and a thin portion of it probably envelopes each of the component filaments of the rope, as it may be termed: the bark is of a leathery substance, and includes a number of small *spicula*: its outer surface is sandy: it is furnished with large, distinct, flat-topped tubercles, from which the polypes are doubtless emitted, as they are from the somewhat similar tubercles of the bark of the genus *Eunicea.* Towards the lower end of the stem the crust is discontinued; and this part is imbedded in a species of *Sponge*, which, if essential to the coral, is, however, independent of it, the sponge occurring without the coral, but the coral not having yet been found without the sponge. The coral seems to be affixed only by the intervention of the sponge, and is not flattened out at the base, like *Gorgonia*, for attachment to other bodies. In *Pennatula*, which is affixed by the insertion of its lower undilated end into yielding substances, the polypiferous crust is continued to the extremity of the stem, and does not cease, like that of the *glass-rope Coral*, at the point of immersion.

Mr. Gray remarked that this *Coral* is peculiar, as being the only body, the animal nature of which is undoubted, that is yet known to secrete silica; the *spicula* and *axis* of all other *Corals* which have

fallen under his observation being purely calcareous: he has not, however, yet had an opportunity of examining the *Gorgonia Briareus*, the axis of which is described by Ellis as consisting of numerous little purple glossy needles, but in the nearly allied *Alcyonium asbestinum* (the *spicula* of which closely agree with this description) he has ascertained that the *spicula* are calcareous. In the siliceous nature of its *spicula* the *Coral* in question agrees with some of the *Sponges*, *Tethyæ*, &c.

Mr. Gray stated that this curious production had occupied much of his attention several years since, and that he had delayed the publication of his views respecting it, in the hope of being enabled, by the acquisition of more copious materials, to clear up some points which did not appear to him to be, at that time, capable of satisfactory elucidation. He characterized it as the type of a new genus.

HYALONEMA.

Corallium simplex, subcylindricum, ad basin attenuatum et in *Spongiâ* immersum, supra basin cortice coriaceo tuberculato tectum; tuberculis sparsis, depressis, polypiferis. *Axis* e spiculis numerosis, elongatis, filiformibus, subcontortis, siliceis constans.

Polypus ignotus.

HYALONEMA SIEBOLDI.

Hab. apud Japoniam, *Dr. Siebold.*

Specimens are contained in the British Museum, to which they were presented by John Reeves, Esq.; in the Museum at Leyden; and in the collection of Mr. Gray; the latter having been purchased from the Dutch Museum, through the kindness of Dr. De Haen. A few fibres of the axis formed part of the Sloanean Collection, when it was originally acquired for the British Museum, but their nature was altogether unknown.

ENTOMOLOGICAL SOCIETY.

At the meeting on the 3rd of August, a variety of remarkable insects, and insect productions were exhibited by various members, including the vegetative Wasp of the West Indies, spider silk obtained from the *Nephila clavipes* of the same country, exhibited by Mr. Hearne; and the following memoirs were read: Note relating to the Beetles observed in unrolling a Mummy at Belfast, by Robert Patterson, Esq.; Account of the Dead Sea Apple, and the Insect by which it is produced, by Walter Elliott, Esq. It was announced that the address delivered by the Secretary at the last anniversary meeting, on the present state of Entomology, had been published for distribution amongst the members, and for sale to the public.

At the meeting in September, various donations of entomological works were received, and the following communications read: Descriptions of two new Irish Crustacea, by Robert Templeton, Esq., R.A.; Description of a new Hemipterous Insect from the Atlantic Ocean, by the same; Letter from Mr. Engleheart announcing the capture of various rare English Insects.

At the meeting on the 5th of October, a communication from Mr. E. Doubleday was read relative to the entire destruction of a beehive by the moth *Galleria cereana*. Mr. Johnstone, of the island of Grenada, exhibited several living sugar-cane plants attacked by the Cane-fly (*Delphax saccharivora*), and gave an account of the destructive progress of this minute insect in the West India islands. Various remarkable insects were exhibited, and the following memoirs were read: On the internal anatomy of the larva of *Calosoma Sycophanta*, by Dr. Hermann Burmeister of Berlin; Notice of the various entomological subjects brought before the assembly of German naturalists at the meeting in September last at Bonn; by Mr. J. O. Westwood.

The chairman announced that the second part of the Society's Transactions was ready for delivery.

LI. *Intelligence and Miscellaneous Articles.*

M. POGGENDORFF'S VINDICATION OF PROF. FARADAY'S RIGHT OF PRIORITY TO THE DISCOVERY OF DEFINITE ELECTROLYTIC ACTION.

To the Editors of the Philosophical Magazine and Journal of Science.

GENTLEMEN,

I REQUEST the favour of your inserting the inclosed translation of an article by Poggendorff (in his Journal for June 1835) relative to a discovery of Mr. Faraday, by which you will oblige, amongst others,

One of his Pupils,
EDWARD SOLLY, Jun.

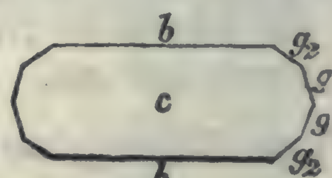
7, Curzon-street, Oct. 14, 1835.

"In the number of the *Annales de Chimie et de Physique* for January * this year, will be found a short paper by Mr. Matteuci of Florence, which all those who have followed the important investigations of Mr. Faraday cannot read without surprise, as they will observe that the author comes forward with no less a discovery than that of the definite electrolytic action, which he explains in a less perfect, but similar way to that of the English philosopher. Mr. Matteuci's paper is dated October 1834; Mr. Faraday's seventh, on the contrary, is dated the 1st of December 1833: moreover, his fourth paper, in which the law before mentioned is already clearly stated, is of the 15th of April of the same year. To whom, therefore, the honour of priority in this case belongs is self-evident. It is possible, though not very probable, that the works of other countries come very slowly to the knowledge of the Florentine philo-

* [It may be proper to observe, in order that this subject may be fully understood, that the publication of the numbers of the *Annales de Chimie* is always several months subsequent to the date they bear; thus, the number for January was not received in London until June.—EDIT.]

sophers; let us, however, recall to mind how very quickly they got intelligence of magneto-electricity: but how the discoveries of Mr. Faraday could be so unknown or wilfully passed over in Paris, that a year later the treatise of Mr. Matteuci could be brought before the public, without a single comment, is truly incomprehensible. To science it is, indeed, indifferent by whom its boundaries are enlarged (although nobody acknowledges the truth of this maxim when his own interest is concerned); but a discovery of such importance as this last of Mr. Faraday's, undoubtedly the only real advance in our knowledge of the chemical action of electricity since 1800, the year of the discovery of the decomposition of water by the battery, such a discovery does at least require some gratitude towards its originator, and the public acknowledgement of his well-founded right of priority is surely the smallest tribute of thanks that can be paid him."—*Poggendorff.*

KUPFER-ANTIMONGLANZ.



This mineral has been discovered by M. Zinken, in the Wolfsberg mine, already celebrated for the specimens of antimony glance, bournonite, zinkenite, and rosenite or plagionite, which it affords. The specific gravity of kupfer-antimonglanz is 4·748, hardness 3·5. Lustre, metallic; colour, lead grey—iron black. Form, prismatic; cleavage parallel to *c* and *b*, the latter being very perfect. $g\,g = 135°\cdot12$, $g_2\,g_2 = 111, \ldots\, g_2\,b = 112\cdot24$, $g_2\,b = 129\cdot30$, $c\,b = 90°$; *b* is streaked parallel to its intersection with g_2. According to H, Rose the composition of this mineral is expressed by the formula $\underline{\overset{\prime}{\mathrm{Cu}}} + \underline{\overset{\prime\prime\prime}{\mathrm{Sb}}}$.—*Poggendorff's Annalen.*

REMARKS ON A PECULIAR STATE OF POLARITY INDUCED IN SOFT IRON BY VOLTAIC MAGNETISM. BY E. M. CLARKE, MAGNETICIAN.

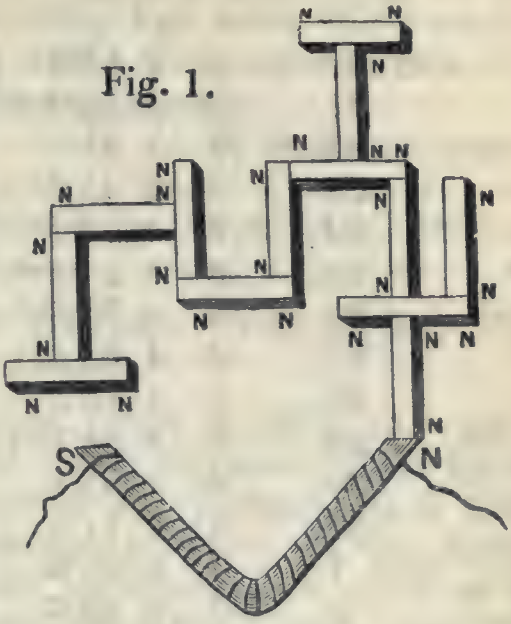
To Richard Taylor, Esq., F.L.S., &c.

SIR,

Having in the year 1833, being occupied in various experiments to produce locomotion by means of voltaic magnetism, I observed various interesting facts; one was, that when a bar of soft iron was placed in contact with one limb of a voltaic magnet (commonly called an electro-magnet,) it partakes of its peculiar polarity N. or S. throughout, as indicated by the invariable position of a magnetic needle whenever brought in approximation with it, an effect strikingly at variance with received opinions on the subject. This law appears to hold good, as far as I have experimented, with several additional bars in various trying positions, as may be seen in fig. 1.

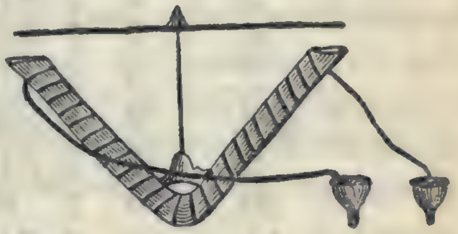
Another fact, which has since created some interest, was also discovered by me whilst experimenting with the same apparatus. I observed that when the compass needle was placed above the poles of the voltaic magnet, as in fig. 2. on breaking the connexion with the voltaic battery, the needle was suddenly deflected half a revolution from its previous position; on restoring the connexion the needle completed its revolution in continuation: by timing the operation a continuous rotatory motion was produced in the needle in various positions (and also in several needles at the same time) adjacent to the voltaic magnet.

Fig. 1.



This rotatory effect is well known to all who have repeated Oersted's original experiment, in which the voltaic wires alone are used to affect the needle.

Fig. 2.



This rotatory motion was applied lately (and described in your valuable Journal for July) to an apparatus copied from that constructed by Mr. Saxton, which construction is merely a modification of one invented by Professor Ritchie, and exhibited two years ago by his assistant (my late talented and ingenious friend Francis Kirby of the London University). Professor Ritchie described his apparatus in the Philosophical Transactions, part ii. p. 319. The only difference between them is, that invented by the learned Professor makes the voltaic magnet rotate, while that of Mr. Saxton causes the steel magnet to rotate.

39, Charles-street, Parliament-street, August 1835. E. M. Clarke.

LETTER OF PROFESSOR ROSENBERGER TO THE EDITOR OF THE ASTRONOMISCHE NACHRICHTEN; WITH AN EPHEMERIS OF HALLEY'S COMET.

The positions refer to the mean equinox of the 16th November, 1835. Regard has already been had to the aberration, through the change of time, which is the foundation of the calculation (mean noon Berlin). The elements are those transmitted on the 6th of October, namely,

$$\log. a = 1{\cdot}25498073, \qquad e = 0{\cdot}96744336,$$

$$- T = \text{Nov. } 16{\cdot}05351 \text{ m. t. Berlin.}$$

$$\pi = 55^\circ\ 11'\ 21''{\cdot}40 \qquad \omega = 110^\circ\ 43'\ 59''{\cdot}42 \qquad i = 162^\circ\ 9'\ 48''{\cdot}53.$$

Ephemeris of Halley's Comet.

Date.	M. T. Berlin. H. M. S.	R. A. ° ′ ″		Decl. ° ′ ″
Oct. 20.	0 2 53·2	252 27 42·2	+	8 21 50·2
21.	0 3 10·2	253 47 17·7		5 29 10·1
22.	0 3 27·5	254 51 46·5		3 4 23·1
23.	0 3 45·2	255 44 23·0	+	1 1 37·1
24.	0 4 3·2	256 27 35·7	−	0 43 36·5
25.	0 4 21·3	257 3 11·9		2 14 43·9
26.	0 4 39·6	32 31·7		3 34 24·1
27.	0 4 57·9	56 35·5		4 44 40·5
28.	0 5 16·3	258 16 9·6		5 47 10·1
29.	0 5 34·7	31 50·9		6 43 10·3
30.	0 5 53·1	44 8·2		7 33 42·7
31.	0 6 11·5	53 24·7		8 19 37·4
Nov. 1.	0 6 29·8	59 59·4		9 1 35·0
2.	0 6 48·0	259 4 7·9		9 40 9·1
3.	0 7 6·1	6 3·3		10 15 47·8
4.	0 7 24·1	5 56·9		10 48 54·0
5.	0 7 41·9	3 58·4		11 19 47·5
6.	0 7 59·6	0 15·9		11 48 44·7
7.	0 8 17·1	258 54 57·4		12 15 59·7
8.	0 8 34·3	48 9·5		12 41 44·4
9.	0 8 51·4	39 58·7		13 6 8·9
10.	0 9 8·1	30 31·1		13 29 22·4
11.	0 9 24·6	19 52·3		13 51 32·2
12.	0 9 40·7	8 7·9		14 12 45·0
13.	0 9 56·6	257 55 23·4		14 33 6·3
14.	0 10 12·1	41 44·1		14 52 41·3
15.	0 10 27·2	27 15·2		15 11 34·2
16.	0 10 42·0	12 1·8		15 29 48·8
17.	0 10 56·3	256 56 9·1		15 47 28·2
18.	0 11 10·2	39 41·9		16 4 35·4
19.	0 11 23·7	22 45·0		16 21 13·0
20.	0 11 36·8	5 23·1		16 37 23·0
21.	0 11 49·3	255 47 40·5		16 53 7·7
22.	0 12 1·5	29 41·3		17 8 28·5

In transmitting to you the foregoing ephemeris of Halley's comet, in the chance of its being useful, I must beg to apologise for having left you and other observers for two days without an ephemeris, and also to state that I am not yet able to transmit an exact comparison of all the observations made by me in the course of that time. From the very irregular motion of the comet, the task of reducing is unusually laborious. To this were added difficulties which it was not by any means in my power to remove.

As the subsequent observation of the comet may be rendered easier by a more correct knowledge of its position, I have thought it better to complete the accompanying ephemeris previous to any other, in order that it may reach in time those who may require it. I have

still a gap, from the 22nd of September to the 7th of October to fill up; as soon as that is done, I will immediately proceed to a complete comparison of all observations, and think that I shall be ready with it in eight or ten days; and I then hope to be able to send you a more full and detailed communication.

A. ROSENBERGER.

Halle, Oct. 13, 1835.

(*Sent as a circular by Prof. Schumacher from Altona, Oct.* 17, 1835.)

OBSERVATIONS ON THE ASSAY OF SILVER IN THE HUMID WAY. BY M. GAY LUSSAC.

There was lately sent to the Assay Office in Paris an ingot of silver, containing 3-1000dths of gold, which had been stated by one assayer to contain 990, and by another 995 thousandths of silver. Numerous assays made by M. Besseyre, to whom M. Gay Lussac had confided the process, gave $996\frac{1}{2}$ thousandths of silver in the ingot; this proportion, added to the 3-1000dth of gold, gave $999\frac{1}{2}$ as the sum of the precious metals. This result excited our attention, for we had never found the fine silver of commerce so pure as to exceed 997 to 998 thousandths. On the other hand, the cupellation to which the ingot was subjected to determine the quantity of gold which it contained, gave only 990 thousandths of silver, instead of $996\frac{1}{2}$ found by the humid way. These results agree with those obtained by the commercial assayers, one of whom had operated in the humid way and the other by cupellation.

Surprised at so great a difference, M. Gay Lussac made researches as to its cause, and he found that it was owing to mercury contained in the ingot. On adding five milligrammes of mercury and one gramme of pure silver, he found, after dissolving the silver in nitric acid and precipitating it by common salt, that the silver had increased about four thousandths. Instructed by this synthetic experiment, he exposed 50 grammes of the ingot, in a small porcelain retort, to a very high temperature, and he obtained small globules of mercury visible to the naked eye.

The cause of the different fineness obtained by the two processes being thus known, it remained to correct it, in order to give the humid process that degree of certainty which it appeared to have lost by this unexpected circumstance; for although silver containing mercury is very rare in commerce, it is sufficient that the case has happened to put the assayer on his guard, and supply him with the means of overcoming it.

M. Gay Lussac at first thought that the mercury had not been completely oxidated, and that it would then precipitate with the silver as an insoluble chloride; but an assay of pure silver to which six thousandths of completely oxidated mercury were added in nitric acid gave 1005 thousandths, instead of 1000 as ought to have been obtained: this result proves that the mercury was precipitated with the silver.

It was then supposed that the mercury, although at the maximum of oxidation, might have been reduced to the minimum, at the mo-

ment of precipitation, by the nitrous acid produced during the solution of the silver in the nitric acid; manganate of potash was therefore added to a solution of a gramme of silver and six milligrammes of mercury, as long as it was decolorized, and even slightly in excess; but the result was not satisfactory: the quantity of the silver was increased by about five thousandths.

It therefore remained to discover the means of ascertaining the presence of mercury in silver; and this was effected by observing the manner in which chloride of silver when pure, and when adulterated with mercury, is affected by light.

It is well known that chloride of silver blackens the more readily as it is exposed to an intense light, and that even in the diffused light of a room it becomes soon sensibly coloured. If it contains four to five thousandths of mercury it does not blacken, it remains of a dead white; with three thousandths of mercury there is no marked discoloring in diffused light; with two thousandths it is slight; with one, it is much more marked, but still it is much less intense than with pure chloride. With half a thousandth of mercury the difference of colour is not remarkable, and is perceived only in a very moderate light.

But when the quantity of mercury is so small that it cannot be detected by the difference of colour in the chloride of silver, it may be rendered very evident by a very simple process of concentration. Dissolve one gramme of silver, supposed to contain $\frac{1}{4}$ of a thousandth of mercury, and only one fourth of it is to be precipitated, adding only $\frac{1}{4}$ of the common salt necessary to precipitate it entirely. In thus operating, the $\frac{1}{4}$ thousandth of mercury is concentrated in a quantity of chloride of silver four times smaller: it is as if the silver having been entirely precipitated, four times as much mercury, equal to two thousandths, had been precipitated with it.

In taking two grammes of silver and precipitating only $\frac{1}{4}$ by common salt, the precipitate would be, with respect to the chloride of silver, as if it amounted to four thousandths. In this process, which occupies only five minutes, because exact weighing is not necessary, $\frac{1}{10}$ of a thousandth of mercury may be detected in silver.

It is not useless to observe, that in making these experiments the most exact manner of introducing small quantities of mercury into a solution of silver is to weigh a small globule of mercury, and to dissolve it in nitric acid, to dilute the solution so that it may contain as many cubic centimetres as the globule weighs of centigrammes. Each cubic centimetres, taken by means of a *pipette*, will contain one milligramme of mercury.

If the ingot of silver to be assayed is found to contain a greater quantity of mercury, one thousandth for example, the humid process ought either to be given up in this case or to be compared with cupellation.

When the silver contains mercury, the solution from which the mixed chlorides are precipitated does not readily become clear.

Silver containing mercury, put into a small crucible and mixed with lamp-black, to prevent the volatilization of the silver, was heated for three quarters of an hour in a muffle, but the silver increased sensibly in weight. This process for separating the mercury

therefore failed. It is to be observed that mercury is the only metal which has thus the power of disturbing the analysis by the humid way.—*Ann. de Chim. et de Phys.*, tome lviii. p. 218.

ANALYSIS OF PYROXYLIC SPIRIT.—METHYLÈNE.

MM. Dumas and Peligot state that pyroxylic spirit was discovered by Mr. Philip Taylor in 1812, and that he published an account of it in 1822; and they also admit that Mr. Taylor's observations with respect to its properties are perfectly correct, and that they have procured pyroxylic spirit possessing all the properties which he assigns to it.

MM. Dumas and Peligot, in common with most chemists who have within a few years analysed any carburetted hydrogen or any substance containing carbon and hydrogen, have found a new compound of these elements, or rather, to use their own phrase, "nous donnerons le nom *méthylène* à un radical dont il est impossible d'eviter la supposition." Formerly it was deemed to be sufficiently early to name a substance when it had actually been found, but now a name is given on the *supposition* that it must exist. This radical the authors say is a carburetted hydrogen, which is the most simple of all, and is stated to consist of a volume of each of its elements, giving as its composition

4 atoms of carbon........	153·05	or	85·95
4 atoms of hydrogen......	25·00	—	14·05
1 atom méthylène	178·05		100·00

The authors then remark that méthylène, bicarburetted hydrogen, and Mr. Faraday's carburet of hydrogen are three isomeric* bodies, in which the number of the elementary atoms go on doubling, the first C H, the second $C^2 H^2$, the third $C^4 H^4$. Pyroxylic spirit yielded by analysis,

4 atoms carbon	153·05	or	37·97
8 atoms hydrogen	50·00	—	12·40
2 atoms oxygen.......	200·00	—	49·63
	403·05		100·00

It is represented as a bihydrate of méthylène, or

1 atom méthylène....	178·05	or	44·17
2 atoms water	225·00	—	55·83
	403·05		100·00

Pyroxylic spirit when kept in a badly stopped bottle undergoes no change by contact of the air; but when its vapour and atmospheric air and spongy platina are exposed together, much heat is evolved, and formic acid is produced; while alcohol in the same circumstances produces acetic acid. During this action the pyroxylie spirit loses all its hydrogen, oxygen replacing it, so that the bihydrate

* In the commencement of the paper pyroxylic spirit is stated to be *isomorphous* with alcohol; is not this a misprint for *isomeric*?

of méthylène is converted into hydrated formic acid, which change is intended to be rendered clear by the following symbols:

$$C^4\,H^4,\,H^4\,O^2+O^4=C^4\,H^2\,O^3,\,H^2\,O+H^4\,O^2.$$

And thus it is remarked that by the *theory of substitutions*, as it is termed, the pyroxylic spirit loses four volumes of hydrogen and gains two volumes of oxygen. By the bye this theory of substitutions means only, we presume, that when any compound parts with one of its elements and its place is supplied by another, both are in atomic quantities; a necessary result of the doctrine of definite proportions.

Chlorine acts upon pyroxylic spirit in the same way as upon alcohol, but the action is less intense. Dissolve a pound of good chloride of lime in water, put the clear solution into a retort, and add to it an ounce of pyroxylic spirit. When heat is applied, impure *chloroform* is obtained in the receiver; it is to be separated from the water, agitated for some time with concentrated sulphuric acid, and then rectified from barytes in fine powder. Chloroform thus prepared yielded by analysis the same results as chloroform obtained from alcohol, viz.

Carbon........	10·1
Hydrogen.......	0·9
Chlorine	89·0
	100·

Sulphuric acid when distilled with one fourth of pyroxylic spirit is attended with phænomena perfectly resembling those of the distillation of alcohol and sulphuric acid.

From the commencement of the ebullition to the end of the action, gas is abundantly liberated; this is a mixture of carbonic and sulphurous acid gases, with an æthereal gas, which is not acid, and which dissolves entirely in water and burns like alcohol. To separate the acids the mixed gases must be kept in contact with fragments of potash for 24 hours; then there remains the pure æthereal gas, or *hydrate of méthylène*, and which is to pyroxylic spirit what common æther is to alcohol, that is to say, the *bihydrate* of méthylène has lost half its water to constitute the gaseous hydrate, just as alcohol loses half its water to form common æther. It consists of

1 atom méthylène........	178·05	or	61·28
* 1 atom water............	112·50	—	38·72
	290·55		100·00

Hydrate of méthylène is a colourless gas; it has an æthereal smell, burns with a pale flame like that of alcohol. When cooled to 3° Fahr. it does not liquefy; water at 64° dissolves about 37 times its volume, and acquires an æthereal odour and a hot taste; alcohol and pyroxylic spirit dissolve a much larger quantity. Sulphuric acid dissolves it in large quantity, but it separates when diluted with water.

Muriatic acid in its nascent state acts upon pyroxylic spirit and produces *chlorhydrate of méthylène*. The best process for obtaining it is to heat a mixture of two parts of common salt, three of sulphuric

* In the *Ann. de Chim. et de Phys.*, this is by mistake printed 2 atoms.

acid, and one part of pyroxylic spirit. With a gentle heat a gas is obtained, which may be collected over water, and which is pure chlorhydrate of méthylène. The water retains any gaseous admixtures.

Chlorhydrate of méthylène is a colourless gas which has an æthereal odour and a sweet taste. It burns with a white flame in the middle and a green one at the edges; water at 60° dissolves 2·8 times its volume of this gas. In its gaseous state, or dissolved in water, it is a perfectly neutral compound, not acting either upon litmus or nitrate of silver; its properties perfectly resemble those of muriatic æther. It is composed of

1 atom of méthylène..................	178·05	or	28·12
1 atom of chlorhydric (muriatic) acid	455·15	—	71·88
	633·20		100·00

At a red heat this substance is decomposed into muriatic acid and a carburetted hydrogen gas.

Hydriodate of Méthylène.—This is very readily procured by distilling one part of phosphorus, eight parts of iodine, and twelve or fifteen of pyroxylic spirit. The iodine is to be dissolved in the pyroxylic spirit, and the phosphorus is to be gradually added to it in a retort. Rapid action, accompanied with heat and the production of hydriodic acid, soon takes place; as soon as the ebullition is over, the remainder of the phosphorus is to be added with agitation; the retort is soon to be heated, and distillation is to be continued as long as an æthereal liquor is produced.

The residue consists of phosphorous acid, phosphométhylique acid, and phosphorus, and it is quite colourless. The liquor obtained in the receiver is composed of pyroxylic spirit and hydriodate of méthylène; the latter is separated by water, which immediately precipitates it; the weight of it is nearly equal to that of the iodine employed. This hydriodate is not, however, pure; it must be distilled in a water bath from chloride of calcium and great excess of massicot. This hydriodate is, when pure, colourless and slightly combustible; it burns only in the flame of a lamp, and then yields abundant violet vapours. Its density is 2·237, and it boils at from 105° to 125° Fahr. The density of its vapour is 4·883. It is composed of

1 atom of hydriodic acid	1592·00	or	10·06
1 atom of méthylène	178·05	—	89·94
	1770·05		100·00

(To be continued.)

COMPOSITION OF PYROMUCIC ACID.

M. Boussingault states that M. Houton-Labillardière, who first removed all doubt as to the existence of the pyromucic as a peculiar acid, found its composition to be

Carbon..........	52·1
Hydrogen	2·1
Oxygen	45·8
	100·0

Thinking that this analysis required repetition, M. Boussingault examined both the crystallized and sublimed acid, and found them similar in composition. The crystallized acid was in fine lamellar pearly crystals, which were dried by exposure to the air; the sublimed acid was perfectly white; it had a granular fracture, because it fused in the neck of the retort into which it was sublimed. Their composition was as follows:

	Crystallized.	Sublimed.
Carbon........	54·0	54·1
Hydrogen	3·9	3·8
Oxygen	42·1	42·1
	100·0	100·0

As pyromucic acid requires a temperature above 275° Fahr., and as it is of the same composition as the crystallized acid, it is evident that the latter contains no water of crystallization. In order to ascertain in what state pyromucic acid exists in salts, pyromucate of silver was analysed; this salt was prepared by adding pyromucate of lime to a solution of nitrate of silver, and allowing the mixture to stand for several days. The pyromucate was strongly pressed in filtering-paper, and continued for a long time exposed to a temperature of 257° Fahr. By the analysis of this salt, the composition of the acid it contained appeared to be

10 Carbon........	764·4	or	58·7
6 Hydrogen......	37·4	—	3·1
5 Oxygen	500·0	—	38·2
	1301·8		100·0

It appears, then, that when it combines with bases, the sublimed acid loses an atom of water. Hydrated pyromucic acid possesses the same composition as the pyrocitric acid, contained in the pyrocitrates. In fact, pyrocitric acid is composed of

Carbon........	54·1
Hydrogen......	3·5
Oxygen	42·4
	100·0

Ann de Chim. et de Phys., tome lviii. p. 106.

ANALYSIS OF GADOLINITE.

Dr. Thomson and Mr. Steel give the following as the composition of gadolinite:

Silica..............	24·33
Yttria..............	45·33
Protoxide of cerium ..	4·33
Glucina	11·60
Protoxide of iron	13·59
Platinum...........	trace
Manganese..........	trace
Moisture...........	0·98
	100·16

This analysis, it is remarked, gives us the constituents of gadolinite as follows:

12·16	atoms	silica
8·06	—	yttria
0·88	—	protoxide of cerium
3·91	—	glucina
3·	—	protoxide of iron.

Were we to suppose the protoxide of iron to be an accidental ingredient, we might consider gadolinite as composed of

2 atoms silicate of yttria
1 atom silicate of glucina and cerium;

or we might consider it as composed of

1 atom silicate of cerium
4 atoms silicate of glucina
8 atoms silicate of yttria.

If the protoxide of iron be an essential constituent, the oxide of cerium, glucina, and protoxide of iron must be in the state of disilicates.

It is stated that benzoate of ammonia throws down glucina, which benzoic acid does not; and that, with care, the peroxide of iron may be completely precipitated by benzoic acid.—*Thomson's Records of Science,* vol. i. p. 408.

RHODIZITE, A NEW MINERAL.

M. Gustan Rose has discovered a new mineral, to which he has given the name of *Rhodizite,* among the tourmalines of the Berlin Museum. There is a good deal of analogy between it and the boracite; it has the same form, hardness, and colour, and the phænomena accompanying its fusion before the blowpipe, with borax, salt of phosphorus, fluor spar and silicate of soda; it also acts in the same manner on boracic acid, and dissolves with difficulty in muriatic acid. Its peculiar characters are as follow: it colours at first the flame of the blowpipe green, then green and red, and at last entirely red; when put on burning coals its edges are rounded; it becomes white and then opake, and is covered with excrescences as when it is heated with the blowpipe; fused with a small quantity of soda it forms a white enamel; and when the quantity of soda is considerable it produces a transparent glass, which does not crystallize on cooling; lastly, when it is dissolved in muriatic acid, and ammonia and oxalic acid are added to it, a great quantity of precipitate is formed. It is found in granite; and it adheres so strongly to the red tourmaline, that when separated it has impressions of it.—*Journal de Chimie Médicale,* October 1835.

MR. SAULL'S GEOLOGICAL COLLECTION.

We are informed that Mr. W. D. Saull, F.G.S., &c., having recently erected a building to contain his Geological Specimens, including also those that were in the collection of the late Mr. Sowerby, is desirous of informing scientific gentlemen, and his friends generally, that the entire collection is now stratigraphically arranged, and that the Museum is open for inspection every Thursday morning, at Eleven, at his residence, No. 15, Aldersgate-street, City.

October 20, 1835.

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London, and by Mr. VEALL, at Boston.

Days of Month. 1835.	Barometer.			Thermometer.			Wind.		Rain.	
	London.		Boston.	London.		Bost. 8½ A.M.	Lond.	Bost.	Lond.	Bost.
	Max.	Min.	8½ A.M.	Max.	Min.					
Sept. 1	30·205	30·133	29 63	78	39	61	SE.	SE.	...	...
2	30·226	30·205	29·70	82	43	63	SE.	SE.	...	...
3	30·115	29·928	29·53	83	53	66	SE.	calm	0·17	...
4	29·822	29·807	29·19	78	55	66·5	SW.	SW.	·15	0·03
5	30·053	29·851	29·24	78	55	64	NW.	SW.	...	·22
6	30·123	30·081	29·54	79	55	62	SW.	calm	...	...
○ 7	30·092	29·972	29·47	80	50	63	S.	calm	...	...
8	29·805	29·586	29·20	68	48	60	S.	calm	·28	...
9	29·737	29·692	29·10	64	45	55	W.	NW.	·08	·40
10	29·468	29·332	28·90	70	50	54	W.	NW.	·06	·09
11	29·548	29·452	28·85	60	47	54	W.	NW.	·01	·13
12	29·440	29·309	28·94	66	40	51·5	W.	calm	·49	·11
13	29·599	29·451	28·98	67	42	55	SW.	calm	·06	·08
14	29·926	29·846	29·32	71	59	57	SW.	W.	...	...
☾ 15	29·899	29·750	29·27	68	44	64	S.	W.	·20	...
16	29·825	29·751	29·25	66	43	54	S.	calm	·17	·04
17	29·733	29·653	29·27	67	38	53	S.	calm	·01	...
18	29·810	29·112	29·32	69	51	53	S.	calm	·76	...
19	29·595	29·118	28·95	70	55	58·5	SW.	W.	·60	·49
20	29·821	29·627	29·07	69	51	59	SW.	W.	·08	...
21	29·876	29·760	29·34	70	53	56	S.	calm	·28	...
● 22	29·581	29·468	29·03	73	54	60	S.	E.	·09	·12
23	29·749	29·645	29·05	68	53	62	S.	W.	·33	·06
24	29·959	29·822	29·27	63	42	57	S.	W.	...	·11
25	29·994	29·866	29·43	67	42	52	SW.	calm	...	...
26	29·686	29·630	29·15	62	45	55	S.	W.	·69	...
27	29·659	29·472	29·09	62	47	50	W.	calm	·07	·31
28	29·653	29·411	28·85	61	41	47	SW.	calm	...	·42
☽ 29	29·630	29·554	29·02	67	50	47	S.	W.	...	...
30	29·259	29·132	28·72	66	50	56·5	S.	E.	·02	...
	30·226	29·132	29·18	83	38	57·2			4·60	2·61

Remarks.

London.—September 1. Fine. 2. Hot and dry. 3. Fine: cloudy: lightning and rain at night. 4. Sultry: rain. 5—7. Cloudy and fine. 8. Slight fog: overcast: rain. 9. Clear and cool. 10. Windy with rain. 11. Cold and windy: stormy showers. 12. Fine: rain at night. 13. Fine: showery. 14. Heavy dew: fine. 15. Overcast: rain. 16. Slight haze: rain. 17. Fine. 18. Foggy: heavy rain at night. 19. Cloudy: fine: rain at night. 20. Heavy rain: fine: clear and windy. 21. Hazy: rain. 22, 23. Fine: showery at night. 24. Overcast. 25. Very fine. 26. Fine: rain. 27, 28. Fine. 29. Cloudy. 30. Rain: fine.

Boston.—September 1—3. Fine. 4. Fine: rain with thunder early A.M. 5. Fine: rain early A.M. 6, 7. Fine. 8. Cloudy: rain P.M. 9. Fine. 10. Cloudy and stormy: rain early A.M.: rain P.M. 11, 12. Cloudy and stormy: rain P.M. 13. Cloudy and stormy. 14. Fine. 15. Cloudy: rain P.M. 16. Fine. 17. Cloudy. 18. Fine: rain P.M. 19. Cloudy: rain early A.M. 20. Fine. 21, 22. Cloudy: rain P.M. 23. Fine: rain P.M. 24, 25. Cloudy. 26. Cloudy: rain P.M. 27. Fine. 28. Cloudy: rain early A.M. 29. Stormy. 30. Cloudy.

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[THIRD SERIES.]

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LII. *On the Theory of the Parallel Roads of Glen Roy.* By *Sir* G. S. Mackenzie, *Bart. F.R.SS. L. & E. &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE memoir by Sir Thomas Lauder Dick, published in the ninth volume of the Edinburgh Transactions, has left the theory of the parallel roads in Glen Roy, &c. incomplete in only one particular, that of finding a barrier to form lakes. After assuming that a barrier had existed, the nature of what it consisted of presents a difficulty, and the mode of its removal another; for not the slightest trace of a barrier of any sort is to be found. After a conversation with Sir David Brewster on this subject in the autumn, when a party to visit Glen Roy was proposed, a thought came across me while reflecting on the difficulties of the case, which removed all regret that I had never examined these phænomena; and I have deferred my visit, in order that I might have an opportunity of submitting to others the completion of the theory as propounded by Sir Thomas Lauder Dick, and of considering such objections to it as may be made; so that I may be able, when I do visit the spot, to observe to what extent they may be well founded. Another reason for my submitting at present what has occurred to me as the true origin of what has remained so long a geological puzzle, is that others may examine various glens and valleys in Scotland with the view to ascertain (as I shall not probably have it in my own power to do so soon) whether any of them present conditions in their

form and relative positions similar to the glens of Lochaber, but without any appearance of parallel terraces.

The subject is extremely interesting; for there is doubtless harmony between cause and effect; and there can be no phænomenon in nature ascertained to be the effect of a known cause without its being possible to discover how that cause was brought into operation in reference to the effect. The phænomena of Glen Roy are probably the only ones in existence where the effect at once proclaims the cause, but which at the same time have for a long period wrapped in obscurity the *modus operandi*. If my conjecture shall not prove a happy one, it will, I hope, have at least the effect of reviving a subject of interest to geologists, and leading to more exact reflection upon it.

I must begin by recalling attention to the fact, that a vast body of water has, at a remote period, swept over the face of this part of the earth, in a direction ascertained by numerous facts to have been nearly from north-west to south-east. To whatever cause this great debacle may be attributed, I assume the fact of its having occurred, in order to explain, if possible, the phænomena of Glen Roy. Although the mode in which I conceive them to have been produced is before my mind in a lively manner, so that I almost imagine I see the operation going on, I feel, it is confessed, some fear lest I should not succeed in making it intelligible to others. To those who have seen the parallel roads I hope my attempt will not be altogether unsuccessful.

On referring to the map accompanying Sir Thomas Lauder Dick's memoir, it will be observed:

1. That the great glen through which the Caledonian canal passes has its direction N.E. and S.W., at right angles to the direction in which the waters of the debacle flowed when they covered the mountains.

2. That the glens in which the phænomena of the parallel roads are to be seen have their openings into this great glen facing the N.W., that is, the current of the debacle.

3. That Glen Gluoy and Glen Roy are bent, as it were, from their openings, of which one is directly into the Great Glen, the other into Glen Spean, which is also bent, but not to such a degree. The basin of Loch Treig is bent in the opposite direction.

I must here remark, that until careful levels be taken, and sections made, some appearances that may lead to the statement of objections to the theory I am about to propound cannot be held to affect the question. Relative levels, the elevation of the summit levels all round the district, the compa-

rative width and the steepness of the sides of the glens, and the width of their openings into the glen of the canal, must all be known before my suggestion can be either confirmed or effectively objected to. If the Government could be prevailed upon to order the Ordnance Survey of this district to be completed, for the sake of assisting to clear up a very interesting geological question, it would do itself honour.

Keeping the topographical position of the valleys in view, it will be perceived that the first effect of the debacle would be to fill them with water.

While the torrent swept over the tops of the mountains, no such effect as the formation of the terraces could take place. But as soon as the waters subsided below the summit level, they would cease to flow towards Strathspey, and their current would be changed into two, one directed towards each end of the great glen.

As long as a supply came from the N.W. all the glens in question would be lakes, with surfaces comparatively still. They would be to the waters of the debacle, what the sea lochs are to the sea.

Thus, then, my conjecture is, *that the waters themselves formed the barrier to these lakes.*

It will be easily conceived that as soon as the waters had subsided to a certain extent, powerful currents and waves would be produced by the opposition of the unequally elevated surface of the land.

Great waves would be produced at both ends of the great valley where it expands, and these, by their action and reaction along the line of the great glen, would agitate the lakes in the other glens, and cause the water in them to subside, sometimes slowly, sometimes quickly, and thus to fulfill every condition necessary for the deposition of the terraces. We must keep in mind that amidst all the agitation, the waters were in the mass gradually subsiding.

Thus the agitation necessary to produce the terraces is easily imagined; and the gradual subsidence of the mass of waters, interrupted occasionally by back waves caused by the meeting of currents and the ocean tides, and agitation at the outlets, will probably account for the appearance of successive terraces to every mind accustomed to contemplate the vast operations of natural causes that have turned and overturned this world of ours.

One difficulty will, however, immediately present itself when the mass is inspected, that while there are three terraces in Glen Roy, there is but one in the other glens, and that at the lowest level. This, however, may hereafter be accounted

for by the difference in the extent, and other circumstances which have not yet been noticed, that may have modified the action of the waters. These, however, may not perhaps be ascertained until a somewhat laborious survey shall have been made. As I see but little prospect of this, I judge it best not to delay sending you this communication, because I flatter myself it may give a fresh interest to Glen Roy, and lead to satisfactory explanations of many phænomena obviously the result of the action of water, but somewhat puzzling on account of the difficulty of finding a barrier. There are numbers of examples of terraces of gravel indicating a certain operation of water. In all such cases, if the geologist will look upwards, and consider the effects of hills and valleys on a subsiding debacle, an explanation will readily be found.

I am, Gentlemen, yours, &c.

Coul, Nov. 9, 1835. G. S. MACKENZIE.

LIII. *On certain Peculiarities in the Double Refraction and Absorption of Light exhibited in the Oxalate of Chromium and Potash.* *By* SIR DAVID BREWSTER, *K.H. LL.D. F.R.S. &c.**

THIS remarkable salt was put into my hands about the end of the year 1832, by Dr. William Gregory, of Edinburgh, to whom I have been indebted for much kind assistance in carrying on my inquiries respecting the action of coloured bodies in absorbing definite rays of the spectrum. A very brief examination of its optical properties was sufficient to indicate its more obvious peculiarities, and a short notice of these was published at the time. Having received, however, from Dr. Gregory a very fine group of well-formed crystals, and having had an opportunity in the spring of 1833 of observing their action upon the spectrum, both in their solid state and in the state of aqueous solution, I am now able to present to the Society a general view of the results which I obtained.

The oxalate of chromium and potash occurs in flat, irregular, six-sided prisms. The two broadest faces are inclined to each other like the faces of a wedge, whose sharp edge is the summit of the crystal. These faces are considerably rounded, being parallel near the base, and inclined to each other about

* From the Philosophical Transactions for 1835, Part i. This paper was received by the Royal Society, January 27, and read February 12, 1835. An abstract of it was given in Lond. and Edinb. Phil. Mag., vol. vi. p. 305. See also p. 134 of the same volume.

three degrees at the apex of the prism. The incidence of the broad faces upon the adjacent faces of the prism is about 140°, and therefore these faces are inclined to one another at an angle of $180°-148°\times 2 = 64°$. The crystal is terminated by four minute planes equally inclined to the broad face and the axis of the prism, but two of these faces often disappear, and the crystal terminates in an oblique edge in place of a triangular apex.

If we call A A′ the broad faces of the crystal, *m*, *m*′, *m*, *m*′ the other four faces of the prism, and *o*, *o*′, *p*, *p*′ the faces on the summit, the following are the angles which they form with each other.

Incidence of A upon A in a line passing through the axis of the prism	5°	10′
———— A upon *m*, and A′ upon *m*′	148	0
———— *m* upon *m*	64	0
———— A upon *o*, and A′ upon *o*′	112	10
———— A upon *p*, and A′ upon *p*′	112	10
———— *o* upon *o*′, and *p*′ upon *p*′.........	50	10
———— A upon A′ over *o*, *o*′ or *p*, *p*′ ...	4	36

The crystals of oxalate of chromium and potash are, generally speaking, opake; and at thicknesses not much greater than the twenty-fifth of an inch they are absolutely impervious to the sun's rays. In this state their colour, seen by reflected light, is nearly black; but their powder is *green* in daylight, and of a *French grey* colour by candlelight. In the smaller crystals, which are generally the best formed, the colour both of reflected and transmitted daylight is *blue*, but that of candlelight is *purple*. I have not been able to find any distinct traces of cleavage.

This salt possesses a powerful double refraction, which is no doubt related to two axes. In reference to the axis of the prism the double refraction is *negative*, like that of calcareous spar. The greatest refractive index is about 1·605, and the least about 1·506, reckoning from a line near the boundary of the blue and green rays.

One of the most remarkable properties of this salt is the difference of colour in the two images formed by double refraction. At a certain small thickness the *least* refracted image is *bright blue*, and the *most* refracted image *bright green*, in daylight, or *bright pink* in candlelight. The *blue* contains an admixture of green when analysed by the prism, and the green an admixture of red, the red predominating over the green in candlelight. At greater thicknesses the blue becomes purer and fainter, and the green passes into red; and at a certain

thickness the least refracted blue image disappears altogether, and the most refracted image is olive green. At still greater thicknesses this image disappears also, and absolute opacity ensues.

When the crystal is exposed to polarized light, with its axis in the plane of polarization, the transmitted light is *green*; but when the axis of the crystal is perpendicular to that plane, the transmitted light is *blue*.

When the oxalate of chromium and potash is dissolved in water its double refraction disappears, in consequence of the particles being released from the force of aggregation by which they are held together in the solid state, and by which double refraction is produced. The solution, however, exhibits the same general action upon light as the solid. At moderate thicknesses its colour is a dark blueish green by daylight, and a bright blood red by candlelight; but when we increase the thickness of the fluid it becomes of a *blueish pink* by daylight, and of a deeper *blood red* by candlelight, the *red* rays continuing to increase both in day- and candle-light, as we lengthen the path of the ray through the solution.

The most remarkable property of the oxalate of chromium and potash, and the one on account of which I have submitted this paper to the Royal Society, is its specific action upon a definite red ray lying near the extremity of the red portion of the spectrum. This is a property which is not possessed by any solid or fluid body with which I am acquainted, although I have submitted some hundreds of coloured bodies to direct experiment. Like all coloured bodies, the oxalate under our consideration exercises a general absorbent action on the spectrum. The smallest thickness of it, in which colour is scarcely discernible, attacks the *yellow* rays of the spectrum on the more refrangible side of the line D of Fraunhofer. As the thickness of colour of the solution increases, the *violet* rays are absorbed, and also all the *yellow*, *orange*, and less refrangible *green*, till the whole space D E, and part of the spaces on the other side of the lines D, E, are wholly destroyed. In this state the prism gives two distinct images of objects, viz. a *red* and a *greenish blue* image, which are considerably separated. As the absorption advances, the *green* on the blue side of E, and the *blue* on the violet side of F, gradually disappear, till a *pure blue* image about F alone remains, and this too wholly vanishes by an increased thickness of the solution, leaving the red rays unabsorbed.

While these changes are going on throughout the spectrum, a specific action is exerted upon a red ray between A and B of Fraunhofer, and in that very part of the spectrum over which

the solution exercises no general absorptive action. The sharp and narrow black band which is thus formed constitutes a *fixed line in all artificial lights*, and also in solar- and daylight, which will enable philosophers to measure the refractive powers of all bodies in reference to this line with an accuracy which could not otherwise be obtained, unless by the use of fine prisms of the refracting substances, which in most cases are unattainable.

In order to render this line or band of real use in practical optics, I have endeavoured to fix its place with as great accuracy as possible. Between the lines A, B of Fraunhofer there is a group of lines nearly bisecting the space A B, which he has marked a in his map. The dark band lies in the space Ba; and if we designate it by the letter X, its position is such that $BX = \frac{1}{3} B a$, or the index of refraction in the *Water spectrum*, of the rays which are absorbed at the band X is almost exactly 1·330701, the temperature of the water being 65° of Fahrenheit.

The relations of this salt to common and polarized light may be readily examined and finely exhibited by placing upon a plate of glass a few drops of a saturated solution of it in water. If the crystals are slowly formed they will be found of various thicknesses, each thickness exhibiting a different colour, varying from perfect transparency, through all shades of *pale yellow*, *green*, and *blue*, in daylight, and through all shades of *pale yellow*, *pale orange*, *red*, and *blue*, in candlelight.

Belleville, by Kingussie, March 21, 1835.

LIV. *Some Remarks on the Laws of Magnetic Attraction. By* R. W. Fox.*

I HAVE shown in a paper which has been published in the Lond. and Edinb. Philosophical Magazine, Third Series, vol. v. p. 1. that the reciprocal force of two magnets, when placed within certain very small distances from each other, will vary in the simple inverse ratio of the distance; and that the magnetic elements, or at least pulverized loadstone, or other ferruginous particles adhering to the poles of the magnets, will, under such circumstances, become arranged in parallel straight lines; thus explaining, as I conceived, the cause of the existence of this law,—such parallel lines being, as it appeared to me, incompatible with the law of the inverse ratio of the square of the distance.

* Communicated by the Author.

The magnets I employed exerted only *one half* their force when from contact they were separated about the *two thousandth* of an inch from each other; *one quarter* their force when the distance between them was *one thousandth* of an inch; *one eighth* when it amounted to the *five hundredth* part of an inch; and so on, in the direct inverse ratio of the distance, until they were *one eighth* of an inch or more asunder; and as they were still further removed from each other, the law of attraction gradually approximated to the inverse ratio of the square of the distance.

It is well known that a magnet has a portion of its lateral force suspended when its poles are made to approach the dissimilar poles of two other magnets, and that it is reduced to a state of comparative neutrality when those poles are brought into actual contact; and this is doubtless the case with the numerous minute magnets of which loadstone dust consists when their tendency to diverge is overcome, or rather superseded, by strong opposite attractions, and they become arranged in parallel straight lines. This arrangement takes place at greater distances with strong magnets than with weak ones, and in like manner the law changes at greater distances in the former case than in the latter. The loadstone dust seems to explain the arrangement of the invisible magnetic elements, supposing them also to possess polarity; and that they do, may be inferred, not only from analogy, but still more strongly from the law which I have shown to prevail between magnets at very small distances from each other; for this phænomenon cannot, I think, be reconciled with the idea of forces emanating in all directions from a centre or centres, whilst it seems to be in strict accordance with the assumed accumulation of energy at the opposite poles of the magnetic elements.

Having more than a year ago announced the existence of the law in question as a general one throughout a considerable series of minute distances, and also endeavoured to show the cause of it, I have felt some hesitation in again recurring to the subject, but I have ventured to do so, conceiving that it will be found to merit more attention than it has hitherto obtained: and when I consider the phænomena of crystallization and those of the polarization of light, &c., I am inclined to believe that even molecular attraction or affinity may, within certain distances, be governed by similar laws; and this opinion, which I have long entertained, has been strengthened by the beautiful experiments in illustration of the laws of electrical attraction which my friend W. S. Harris exhibited in Dublin before the Physical Section of the British Association.

LV. *Extracts from a Prize Essay on Iodine.* By JAMES INGLIS, *M.D.**

ALL authors, from its discoverer down to the latest periods, have considered iodine as a non-conductor of electricity; but Mr. Kemp, of Edinburgh, two years ago satisfactorily showed that this opinion was erroneous. I have found too that when iodine in a state of perfect dryness was fused, it became a conductor. All that is necessary is to fuse the iodine in a tube closed at both extremities, and having a platinum wire hermetically sealed into each end, and connected to each other internally, *only* through the medium of the fused iodine. When one of these is placed in attachment to the end of a galvanic battery, and the other is dipped into water, the water is instantly decomposed on the formation of the galvanic circle†.

We ought never to call any substance a non-conductor until it be first tried in that state which brings its component particles into close approximation; for the best conductors in a granulated state cease to transmit electricity, and even aluminum "in its fused state becomes a conductor of it‡."

Iodine imparts to water a decided brown tint; and M. Ampère found that this colour entirely disappeared after the action of solar light upon it. Gay-Lussac found that both hydriodic and iodic acid existed in this decolorized water, so that the chlorine solution reproduced the brown tint; but he does not consider this action of iodine on water to be dependent on solar light: he ascribes the deprivation of colour to the evaporation of the iodine which was held in solution by the hydriodic acid formed at the time when the iodine was added§. I found that it was deprived of colour, whilst the bottle containing the solution was tightly stoppered, so that the evaporation of iodine could not be the cause of that change; but I found also that a solution of iodine in water may be kept for any length of time (the period I kept it was eighteen months), exposed to the full action of the sun's rays, without an alteration of colour. All that is requisite is to exclude the air; for when, instead of filling the bottle full of the solution, I only made it half full, the other half being atmospheric air, then in a very short time the colour was destroyed; and this is not from the

* Communicated by the Author.

† [It is not quite clear from this statement whether the experiment was made while the iodine retained the liquid form, or after it had assumed the dense solid state by cooling. Perhaps Dr. Inglis will supply the required information, as it may have some important bearings.—E. W. B.]

‡ Antony Tod Thomson's Therapeutics, vol. i. p. 139.

§ *Annales de Chimie*, vol. xci. p. 155. Berzelius, *Traité de Chim.* 1829, p. 298.

volatilization of the iodine, for none could be observed upon the sides of the upper half of the bottle, nor could the original colour be restored on shaking the bottle, which would have happened had the iodine been present. I had no good means of ascertaining whether nitrogen remained in the upper half of the bottle, but as air seems to be necessary, it is most probable; for supposing that three atoms of water were decomposed, this would give rise to three atoms of hydriodic acid, whilst two of oxygen from the air might combine with the three of the decomposed water, and one atom of iodic acid would result; thus accounting for the presence of these two acids.

Pretty good and regular octohedral crystals of iodine may be sometimes found imbedded in the sulphur which has been deposited during the formation of hydriodic acid by the transmission of sulphuretted hydrogen through iodine suspended in water. And very large fern-shaped [aggregates of] crystals, nearly two inches in length, I obtained by saturating a hot alcoholic solution with a large proportion of iodine; on cooling, a splendid large crystallization presented itself.

When chlorine gas is transmitted through a thickly saturated solution of iodine in alcohol, the dark colour is changed into that of a straw yellow, and a white precipitation takes place, which at first I thought to be iodic acid; but this precipitate is more permanent, not deliquescing in a moist atmosphere, nor is it in even a moderate degree soluble in water, thus differing from iodic acid. When nitric acid is added to this yellow liquid, no change at first takes place; but in a short time a considerable reaction is observed, and heat is generated, and brisk ebullition ensues; iodine being at the same time volatilized and deposited in crystals throughout the liquid. Sulphuric acid instantly decomposes it, liberating iodine. Ammonia in its aqueous solution instantly precipitates the brown detonating iodide of nitrogen. Phosphorus in contact with it quickly liquefies, and the fluid assumes the deep iodous colour. The alcoholic solution of potassa throws down a dense white precipitate inclining to pink, and the fluid assumes both the taste and smell of periodide of carbon. * * *

Mr. Connell's process for obtaining crystals of iodic acid is decidedly the best, and if continued for two or three weeks yields at length beautiful large crystals†. According to Bonsdorff, tincture of Brazil wood is a test for iodic acid, casting with it a dirty yellow colour, which remains unchanged.* * *

Sementini on the discovery of an acid composed of oxygen and iodine, formed by the application of heat to a mixture of an excess of chlorate of potassa and iodine, called it iodous acid;

† [An account of Mr. Connell's process will be found in Phil. Mag. and Annals, N.S. vol. x. p. 235.—Edit.]

but in a letter to Mr. E. Daniell of the 20th of April 1833†, he seems to consider that compound as an oxide of iodine, and that by an admixture of it with iodic acid, an iodous acid properly so called results; this last is a liquid of an amber colour, and is the result of definite proportions only. From my experiments I think that the latter is the correct opinion; but my iodous acid is solid, whilst both of the former combinations are liquid. I most punctually followed the directions given by M. Sementini for the formation of his first iodous acid. I had the salt in excess, and on the application of heat there soon appeared a dense cloud of white vapour, which condensed into white flakes in the receiver. Soon after this the darker iodous vapours appeared, which also condensing, trickled down, and remained liquid in the bottom of the vessel for some time. I poured some of this into a small phial, and soon after it concreted into a solid mass. It fused at a very low temperature, and did not solidify again for a long time. It was not till after standing for several days in a well-stoppered bottle that crystallization took place. By holding the preparation between the eye and the light, the small but regular crystals of iodous acid were seen of a reddish brown colour. When alcohol was added to a portion of it, a solution of an amber colour resulted, and at the same time iodic acid was precipitated. Hence I supposed at first that two compounds were formed in this process, and both *solid*; one of a dark brown amber colour, having a great affinity for water, being soluble in it, and in alcohol; whilst the other, viz. the iodic acid, is insoluble in the latter, and consequently precipitated. Since reading Sementini's last letter, however, I think that the solid iodous acid just described is the result of the union of iodic acid and an oxide of iodine, and is the same as his last iodous acid, only more concentrated. The reason that Sementini's result was different was this; he did not exclude the air from his, consequently being exceedingly deliquescent, it attracted moisture, and he only saw it in its liquid form, whereas it is evident from the preparation now in Dr. Hope's possession that it is a solid, having somewhat of an acicular crystalline form, exceedingly fusible by heat, and absorbing moisture rapidly. When this acid and alcohol are distilled together, iodous æther comes over of an amber colour and a peculiar odour. The residue in the retort being still further acted on by heat is decomposed, white vapours escape, and iodine remains behind, which crystallizes on cooling. * * *

Besides the two chlorides already admitted by chemists,

† [Sig. Sementini's letter was published in Lond. and Edinb. Phil. Mag., vol. iv. p. 392.—Edit.]

Mr. Kane of Dublin has described a third, which has not yet been got in an isolated state: and I think a fourth may fairly be added, for in all cases when I brought chlorine in a perfectly dry state into contact with iodine, also freed from moisture, I found that constantly the first step in the process was the liquefaction of the iodine. Now what is this but a change in form of both the elements? Their characters are changed, they have now assumed a new state of existence, and what more, then, is necessary to constitute a chemical compound? This being the lowest state of chloridation, the dark liquid compound formed may be called a subchloride of iodine. As the action proceeds, more chlorine is absorbed, and the dark reddish brown compound of Gay-Lussac is formed, which may be called the sesquichloride; and lastly, the perchloride, or the chloriodic acid of Sir H. Davy, of an orange yellow colour, results. * * * *

When iodine is added to the chloride of sulphur a compound is formed having many properties in common with bromine. This artificial bromine is decomposed by galvanism, whilst the true element is not. We are certain that there have been abounding in the waters of the ocean from time immemorial these three principles,—chlorine, sulphur, and iodine; may not, then, the slow and long-continued action of these on each other in their purely nascent state, account for the undecomposable nature of the natural bromine? * * *

Mr. Kemp discovered a very beautiful process for the liquefaction of sulphuretted hydrogen: he found that if dry persulphuretted hydrogen be introduced into a liquefying-tube, it slowly resolves itself into liquid protosulphuretted hydrogen, whilst sulphur in crystals is deposited. If previously there has been introduced into the end of the tube iodine in a dry state, then the protosulphuretted hydrogen, when it comes over upon it, dissolves it rapidly, and a dark yellowish brown coloured liquid results. If now to this there be added the least possible proportion of water (which is accomplished by a peculiar bend in the tube), instant reaction takes place, sulphur is deposited, and hydriodic acid in a most condensed and liquid state results. It is only necessary that a trace of water be present to commence the decomposition of the former brown compound, which I suppose to be the hydrosulphuret of iodine; for when this once commences, it goes on to any extent, and the liquid hydriodic acid formed may be called almost anhydrous. It boils by the heat of the hand like other condensed gases; it is of a yellowish colour, and resembles somewhat liquefied chlorine. * * *

[To be continued.]

LVI. *An Inquiry into the Nature of the Structure of Rocks.* By Henry S. Boase, *M.D., &c.*

[Continued from p. 383.]

BUT let us endeavour to advance a step further in the practical application of the new system to rocks *in situ*, to the roofing-slate, for example, in one of the most extensive slate quarries of Wales, where the characters of the rock are all fresh and well defined, on account of recent and extensive excavations. Is this a *slate*; that is, has it a *perfect cleavage*? According to the old phraseology this would be deemed a very simple question; but now it can only be answered by another interrogatory, What is the direction of the strata? Prof. Sedgwick gives the following answer: "Of all places a slate-quarry is often the very worst for determining the stratification of the neighbouring country." The reason of this we are told, in another place, is because "the structure of the rock has been so modified that traces of its original deposition are quite obliterated; and this remark does not apply merely to single quarries, but sometimes to whole mountains." Without stopping to inquire by what indications it was ascertained that the marks of deposition once existed, or by what possible modification of structure they could be obliterated, we must proceed with the question under consideration, viz. whether the Welsh roofing-slate has a true cleavage? A little further on the Professor informs us that "sometimes all these means (the ordinary means of discovering strata) fail, and we may ramble for miles among mountains of slate without seeing a single trace of their original stratification."

It therefore follows, that the apparently self-evident question whether a rock be fissile or has a slaty structure, must be often left undetermined, since a few miles' ramble will frequently bring us not only on rocks having different bearings, but even on entirely new formations. And the difficulty will be greatly enhanced if the geologist should unexpectedly alight on a good slate-quarry situated in an igneous formation, such as the Roche Tiulière in the Mont Dove; for the terms *slate, flagstone*, and *laminæ* of the new vocabulary would be inapplicable, in as much as the laminations or leaves can, in their position, have no reference to that of strata, in an unstratified rock.

It is not stated in the definition of the term cleavage, but it may be gathered from the details (at pp. 471 and 473 Trans. Geol. Soc. Second Series, vol. iii.), that the transverse cleavage

"may be carried on *indefinitely*, or at least as far as the operation is not interrupted by a mere mechanical difficulty." Again, "should any one assert that the subdivision of slate-rocks into rhombohedral solids implies three planes of cleavage, we might reply, that such solids are not capable of *indefinite* subdivision into similar solids, except in one direction, viz. that of true cleavage; and in this way we may generally distinguish the true cleavage planes from the joints." Now we are also informed (at p. 477.) that "the cleavage planes pass alike through all the strata, the coarse beds and the finer, the twisted and the straight." We might be led from this to conclude that all these strata are susceptible of indefinite subdivision; but such a mistake would be corrected in the page following, which states that "where the slaty cleavage is very perfectly brought out, the structure of the rocks always makes an approach to homogeneity; where the quartzose beds of coarse greywacké abound very much, the cleavage is seldom very perfect, or is at least chiefly confined to particular strata; and where the coarse beds predominate, the slaty structure almost entirely disappears." This is contradictory to the preceding statements, for it shows that cleavage planes do not pass alike through all the strata, that they are not indefinite, and that they are often as far apart as the rhombohedral joints, so that they cannot be thus distinguished from the latter kind of structure. In drawing, therefore, the sections to illustrate this subject, all the lines in the slate should not be extended through the coarse rock, but only one here and there should have been made to intersect all the strata.

It appears, then, that the proposed restriction of "old terms" is not well adapted for practical purposes; and for this simple reason, because the definitions of these terms require a preliminary knowledge of the direction of strata; a proposition which cannot always be solved, and even when ascertained to the satisfaction of one party, may be disputed by another as purely hypothetical.

We therefore contend that the slaty cleavage of rocks has not necessarily any connexion with the direction of beds or strata which have been formed by, and mark the order of, deposition, but that it is, as elsewhere remarked, "a peculiar kind of structure common to all rocks, both igneous and aqueous, and which has most probably resulted from a crystalline arrangement of their particles during the process of consolidation." The Professor is also of opinion that the regular cleavage is to be attributed to crystalline action; but I am not quite sure whether he supposes it to have operated on solid rocks, as seems to be implied by the observation (at p. 477.) that "crystalline forces have *rearranged* whole mountain

masses, producing a beautiful crystalline cleavage, passing alike through all the strata." There can be no doubt that the particles of a solid rock may undergo a new arrangement by the action of heat, if sufficient to overcome the previously existing cohesion; as in the case of crystalline limestones next igneous rocks, of sandstones which have been long subjected to the heat of a furnace, and in similar instances; this, however, is but a secondary or superinduced operation of the attraction of aggregation, after its partial or total suspension in solid masses by the superior force of an antagonist power.

The Professor admits that igneous as well as stratified rocks have also been subject to this crystalline action, for he says (at p. 482.) that "even in formations of true granite we occasionally see imperfect indications of a cleavage," which "has, I believe, in most cases been produced during the passage of the whole granitic mass into a solid state by that kind of compound crystalline force which has produced the transverse laminations of argillaceous schist." Although the indications of a cleavage are imperfect in granite, they are not so in all igneous rocks, since some trappean rocks afford good slates. A perfect cleavage, therefore, can in fact have no such absolute connexion with the mode in which the materials of a rock have been deposited as to make this a criterion whether a rock has or has not a true cleavage, because many rocks are stratified and yet are not fissile. We think, then, that it must be admitted that the cleavage, both of igneous and aqueous rocks, is a crystalline structure; and that a rock must be considered as fissile, as a true slate, when it cleaves into thin laminæ, whether these be transverse to the strata or not. It remains to be seen whether geologists will sanction the proposed innovation on the old nomenclature.

The last kind of structure of which the Professor has treated in the paper under consideration is the "jointed structure", which he is of opinion has had a very different origin from the fissile or slaty structure; the latter having "resulted from the ultimate chemical [query crystalline ?] arrangement of the particles of a rock," whilst the former "seems in most cases to have been produced mechanically, either by a strain upon the rock from external force, producing, more or less, regular sets of cracks and fissures*, or by a mechanical tension on the mass (produced probably by contraction) during its passage from a fluid or semifluid, into a solid state."

It is stated (at p. 480. and 481.) that "many rocks, both stratified and unstratified, are divided into solids of greater or

* [We have printed these sentences as correctly quoted from the Trans. Geol. Soc. by Dr. Boase, but we apprehend that there is some error in the punctuation, and that they ought to stand thus, "producing more or less regular sets of cracks and fissures."—Edit.]

less regularity by parallel systems of fissures or joints," but that "the jointed structure is best seen in unstratified rocks, such as basalt and certain varieties of granite." The joints are certainly very distinctly marked in these rocks, but not more so than in very many stratified rocks; for example, in all the older slates both primary and fossiliferous; and without doubt the jointed structure is much more perfectly developed in the sandstones of many parts of Scotland than in the masses of trap with which they are associated. The slaty structure however, when present, is so conspicuously prominent, that it obscures, or rather diverts the attention from, the joints by which the rock is divided into crystal-like concretions. But the jointed structure will, I think, be found to occur more or less distinctly in all rocks that are completely solidified. It is sufficient however, for the purpose of the present inquiry, that stratified rocks have sometimes the jointed structure; in the slate of Cumberland and Wales, for instance, "besides the planes of cleavage, we may often find one or more sets of cross joints, which, combined with the cleavage, divide the rock into rhombohedral solids."

It is worthy of remark that the regular concretional forms of slate rocks are, almost without exception, varieties of the rhomboid; a figure which is also frequent in porphyry, trap, granite, and other igneous rocks; and it must also be observed that these rhomboids, especially in compact rocks, are likewise jointed diagonally, a fact which may be detected on the face of the weathered rock, or, if much decomposed, by its falling into three-cornered pieces under the blows of the hammer; a fact which is important as accounting for the position of some veins which are disposed diagonally to the principal systems of *regular* and *cross* veins.

How are joints to be distinguished from cleavage or slate planes? The Professor says that "the joints are at definite distances from each other, and a mass of the rock between them has, generally speaking, no tendency to cleave in a direction parallel to them." It is admitted immediately after that there are exceptions to this rule, for "a slaty and jointed structure are often exhibited together; in such a case the rock has fissures or joints and true cleavage planes coincident with each other; and again, when a cleavage is imperfect, it is sometimes only exhibited by parallel planes at definite distances, in which case it may be difficult to say whether the phænomena are to be classed with joints or cleavages."

This difficulty is said to have arisen from one set of joints having been formed mechanically on the planes of cleavage. If this be the case, we would ask how it could be determined that the cleavage passes alike through all the strata in Wales,

whether coarse or fine beds? For if in the coarse rocks the cleavage planes are far apart like joints, how can it be known that the slaty cleavage ever passed through the coarse beds? It may be after all only the joint or supposed mechanical fissure, which in the slate is coincident with some of its lines of cleavage. We are therefore inclined to take a much more simple view of the subject, viz. that the slaty cleavage and joints have been formed by the same cause—that they are both crystalline structures;--not denying, however, that external force may have operated in the direction of any of these crystalline planes, but maintaining that this coincidence has no connexion with the production of the jointed structure.

The Professor seems to think, because the cleavage can be carried to a greater extent on the slaty laminæ than on either of the other planes of structure, that there is no cleavage parallel to the latter. It has already been shown that a true cleavage is allowed to be sometimes at definite distances from each other; therefore the smallness of the space between two parallel surfaces, that is, the thickness of the layer of rock, does not essentially constitute a plane of crystallization, or a perfect cleavage. Now, a large rhombohedral mass of slate may be divided, in the direction of its laminæ, into several thick plates, which can be again broken across so as to afford numerous small rhomboids. These are always developed at the surface of such a rock by decomposition; but can also be disclosed in the perfect unchanged state by mechanical division: and even thin roofing-slates, exhibiting no trace of joints, and therefore no cracks and fissures, may be broken across, in certain directions, so as to give lateral planes inclined to the surface of the slate at considerable angles. Moreover, do not the crystals of some simple minerals afford a perfect parallel; crystals of mica, for example, being foliated in the direction of one cleavage plane, and not in the other; and in such cases would any one maintain that the foliated planes are crystalline, and that the others have arisen from mechanical violence?

This notion of a monogenous cleavage in stratified rocks has been extended by the Professor to some instances of igneous rocks; by which, indeed, he has avoided one difficulty, but augmented his labours by bringing into existence a sevenfold increase of similar exceptions.

The cleavage of granitic rocks, or *grain*, as the Professor terms it, is indicated by parallel laminations, which are allowed to have arisen from crystalline action; the cause, then, of this and of the slaty structure or transverse cleavage is identical. In the granite of Carclaze, near St. Austle, specially quoted to illustrate this opinion concerning the *grain* of ig-

neous rocks, we are told that the slate in contact with the granite "has been modified by a similar crystalline action in passing into a solid state." Now, in the preceding pages it had been laid down that the folia of schists which are parallel to the strata, are not true slates, but laminæ or thin beds; the difference being, that one is supposed to have been formed by crystalline action, the other by deposition. The demonstration of this proposition has therefore failed in the case of the Carclaze slate, and through it in all parallel cases, by leading to the contradictory conclusion that *laminæ* are not *laminæ*, but *slates*; that is, that a perfect or crystalline cleavage is not always transverse to the strata, which is one of the essentials in the proposed definitions of these terms. This result, however, is satisfactory, for it is in accordance with Nature, the inclination of the laminæ of rocks having no fixed relation to that of their bedding or stratification.

For reasons similar to those already advanced concerning the supposed single cleavage of slate, it may be disputed that granite has only one *grain* or plane of lamination. For, since there are no precise limits to the thickness of these laminæ, (as next the slate at Carclaze, they are said to be very thin, while at a distance therefrom they are more largely developed,) we cannot deny that the granite of other places has a *grain* when its layers are parallel to the crystalline planes of the adjacent slate. Now, in all cases these laminations or layers are, by the action of the elements, divided into quadrangular masses, in consequence of the development of the joints; and they may, when the joints are not visible, be cleaved or subdivided not only into thinner layers, but also across their planes, in two directions, so as to produce blocks similar to those formed by atmospheric agency. It must be admitted that those planes which have the same direction as the *strike* of the adjoining strata, and correspond with the laminæ of the slate, are more easily demonstrated; and Mr. Enys has recorded that the workmen can cleave the granite with less power on this line than on the others*. But we do not know any reason why one system of planes should be referred to crystalline action, and the others to mechanical violence; the notion appears only to have been adduced in support of the hypothesis of one true transverse cleavage in stratified rocks, against which we have been contending.

The Professor states that the *grain* (that is, the cleavage in one direction) has had considerable influence in modifying the course of fissures subsequently produced by mechanical force, and by this means he accounts for the regularity of one set of

* [See Lond. and Edinb. Phil. Mag., vol. ii. p. 323. — Edit.]

joints; but what, it may be asked, governed the fracture of the rocks at right angles to these planes, for the cross joints are as regular as the others? And what cause produced the third system of joints, in the cuboidal and rhomboidal masses, which is, in general, but gently inclined to the horizon? Much stress has been laid on Mr. Hopkins's mathematical investigations, which go far, it is said, to prove that "tabular masses of rock elevated by a force from below, must have been exposed to two sets of tensions, which would naturally produce longitudinal and transverse vertical fractures, at right angles to each other." But this does not account for the third system of planes, and more particularly for that which arises from the division of rocks into rhombohedral solids, which happens nine cases out of ten in regularly jointed rocks; indeed, if we have not misunderstood Mr. Hopkins, the formation of such fractures by the tension of the mass from an internal force is a physical impossibility.

As regards the production of "cracks and fissures" by the other mechanical action mentioned by the Professor, viz. by contraction, occasioned by the consolidation of rocks, it is not very evident that this is a sufficient cause. Even in the desiccation of moist clay or starch, often quoted by other writers, contraction seems only to be the effect of the aggregation of the particles into less space, that is, the consolidation is accompanied by contraction; but this is not the cause by which the mass is divided into definite forms. It is doubtful, however, whether such open spaces would occur in sedimentary deposits whilst under great pressure, and liable to the introduction of additional aqueous sediments from the superincumbent unconsolidated mass. Much less can it be admitted that masses which had been in a state of fusion would be rent into cross fissures by the reduction of temperature; in this case, concretionary joints would certainly abound, and might even be visible on a section of the mass; but it does not necessarily follow that these joints should be open, or fissures, for crystalline bodies formed by fusion do not exhibit such appearances.

For the reasons which have now been advanced, I am induced to withhold my assent from the views promulgated by Professor Sedgwick concerning the structure of rocks. Indeed they appear to me to have rendered the subject more complicated than it was previously, by making unnecessary distinctions. I may possibly be in error; and if so, I trust that sound arguments will find me open to conviction.

When the structure of igneous rocks was under consideration, I proposed to divide their kinds of structure under three

heads, the molecular, concretionary, and jointed structures, thinking that there was evidence of these three stages succeeding each other during the consolidation of rocks. It was then also stated that stratified rocks formed from sedimentary deposits, possessed, when solidified, similar structures, effected by the same cause, viz. the agency of the attraction of aggregation. The first stage of consolidation is already accomplished in deposits of sand, gravel, pebbles, and the like; for the molecules are united into distinct bodies: these, in the next place, are combined into various *concretional* forms, such as nodules, spheroids, angular and venous portions, and, perhaps, laminæ and slates, though these last are so intimately connected with the *jointed* structure that I am not inclined to separate them from it. And it may be here remarked, that as the joints, in igneous rocks, sometimes traverse large crystals formed during the first or molecular stage, so in some sedimentary rocks they occasionally intersect the previously existing pebbles and fragments of which these rocks are composed.

On this view of the subject, no distinction is attempted between laminæ that have different positions as regards the beds or layers in which they occur, nor between those which belong to rocks of various origin. And whilst the slaty cleavage and the joints are referred to the same crystalline action, yet it is not denied that the rocks may have been subject to rents and fissures by mechanical violence; but if thus disturbed, it is presumed that the continuity of the rock would be broken in directions parallel to the crystalline planes or joints in preference to any other, as offering least resistance; thus the structure of the mass may be more clearly displayed; but its origin need not be attributed to such convulsions. To sum up the whole, "it matters not whether a consolidated rock has had an igneous, aqueous, or any unknown origin; its particles were once disunited or mobile, either by the repulsive agency of caloric, by chemical solution or suspension in water, by mechanical attrition, or by some other cause: subsequently, however, its particles have been brought within the sphere of their mutual attraction by a reduction of temperature, by precipitation, by great and long-continued pressure, by the percolation of water imparting the requisite degree of motion, or introducing extraneous matter by which the particles are cemented, or lastly, by any two or more compatible causes which can operate in unison to effect the cohesion of its particles."

Before concluding, permit me to make a few remarks on a portion of the Professor's paper in which he has offered some

suggestions without alluding to their having been already propounded.

Thus, at page 483, he observes that "if the *grain* of granite rocks be produced by a modification of crystalline action, similar to that which produced slaty cleavage, have we not some reason to expect that the *grain* of such rocks may be traced along their lines of protrusion? I throw this out as a mere conjecture, suggested by an analogy, and by the fact that the veined structure of the St. Austle granite is nearly parallel to the direction of the Cornish slates."

Now in a paper read before the [Royal Cornwall Geological] Society in the years 1830 and 1831, and published in the Transactions in 1832, I stated that the masses of granite divided into layers by seams or joints, are sometimes perpendicular, but more frequently inclined at various angles from 45° to 80°; and that they only differ from the layers or strata of the adjoining slate, in the one being fissile and the other not; in short, that they both possess the same concretionary structure, the result probably of a peculiar crystalline arrangement of their constituent minerals;—and lastly, that beds of granitic rocks, forming integrant parts of the central masses, sometimes extend beyond the boundaries thereof, and alternate with the slate. Again, in my "Treatise on Primary Geology," I have often dwelt on this topic, showing that the layers of granite vary in composition and alternate together precisely after the same manner as the crystalline slates; that they have often determinate directions, as recorded by several observers in different countries; that even when the joints are not visible they may be cleft most easily in directions parallel therewith, as pointed out by Mr. Enys; and lastly, which is to the point, that "the direction or strike of these granitic beds is placed in a certain and determinate position, which in Cornwall is parallel to the most frequent course of the adjacent schistose rocks." For further details on this subject the 6th, 10th, 12th, and 16th chapters may be more particularly consulted.

The Professor next adds: "It would be well, in a place like Cornwall, to institute a set of direct observations, for the purpose of comparing the grain of the granite with the direction of the nearest metalliferous veins. As so many rocks are intersected by cross joints, nearly perpendicular to their *strike*, we might expect, *à priori*, to find many great 'master joints,' nearly at right angles to the direction of the granitic ridge of Cornwall. At all events, whether such reasoning be good or bad, there are many great 'master joints' or 'cross courses' in that country nearly at right angles to the bearings of the central chain."

Now, it has been a long- and well-established fact in Cornwall, that metalliferous and other veins continue their course uninterruptedly through both the granite and the slate; that the principal veins are generally N. of E. and S. of W., thus having the same direction as the layers of granitic rocks or elvans, and as the strata of the schistose rocks; and also that these veins are crossed by others nearly at right angles. I believe that I first pointed out the correspondence between the directions of veins, and of joints or seams, which intersect both granite and slate, dividing them into regular concretions; this suggestion was published in my paper above alluded to, and thus concludes: "this view of the subject explains why the different series of veins cross each other, and why the veins of each series are respectively parallel." At the Oxford Meeting of the British Association in 1832, I entered at some length into an explanation of this opinion, a brief notice of which will be found in the first volume of the printed Reports, recommending this as a subject deserving the consideration of geologists; and this notion is still further developed in the work which I published during the past year.

This statement shows that observations have been *already* instituted in Cornwall to compare the direction of the veins not only with the layers of granite, but also with those of the schistose rocks; and I may add that a considerable mass of information on this subject has been collected by Mr. Henwood.

I regret that I have been compelled thus publicly to notice a matter in which I am personally interested; but I could not pass over in silence Prof. Sedgwick's omission to allude to what had already been done on the subject of his suggestion and recommendation, without appearing to sanction an infringe- of the "good principle *suum cuique*," which ought always to be maintained in scientific intercourse.

LVII. *A concise Method of determining the Function* X_2 *in the Application of* Sturm's *Theorem. By* J. R. Young, *Esq. Professor of Mathematics in Belfast College.**

THE valuable theorem which M. Sturm has discovered for the separation of the roots of numerical equations, was, till very lately, almost entirely unknown to British mathematicians. Yet the memoir which embodies this discovery, though not printed till July in the present year, was read before the *Académie des Sciences* so long ago as 1829; since which period it has been gradually disseminated throughout

* Communicated by the Author

France, and is now universally adopted, in the Continental colleges, in place of the laborious method of Lagrange.

It is presumed that the small volume which I have recently published upon the theory and solution of equations, as also Mr. Spiller's elegant translation of Sturm's memoir *, will render any detailed account of the theorem itself unnecessary in this place. My object in the present communication is merely to offer a suggestion, for the purpose of facilitating a little its practical application.

The principal operation which Sturm's theorem involves, is that of finding the greatest common measure between the polynomial X, forming the first member of the proposed equation, and its derived function X_1. The series of remainders or successive quotients at which we arrive in the course of this operation, require to have their several signs changed as they arise; and they then form, together with the original polynomials X, X_1, a series of functions

$$X, X_1, X_2, X_3, \ldots$$

from which every requisite information respecting the number and situation of the real roots of the equation $X = 0$ may be readily evolved.

Now, instead of taking the trouble of actually performing the division of X by X_1, in order to obtain X_2, the remainder due to that division after having changed signs, we may find X_2 much more easily by the following rule, viz.

(A.) Having arranged the terms of X_1 so that they may be severally under those involving like powers of x in X, proceed thus: multiply the third term of X by twice the coefficient of the first term of X_1, the next term by three times that coefficient, the next by four times, and so on. From these several results subtract those which arise from multiplying the terms immediately under them by the second coefficient in X: the remainders, taken with contrary signs, will form X_2.

(B.) When the second term of X is zero, X_2 is obtained by simply multiplying the terms of X, commencing at the third, by -2, -3, -4, &c. respectively.

Note.—Instead of using, as the rule directs, the coefficient of the first term of X_1, and the coefficient of the second term of X, for multipliers, we may employ any two factors which will make these coefficients equal and of the same sign as the first in X_1.

An example or two, selected from the volume on equations

* A review of this work will be found in our last number, p. 384.

before adverted to, will show the practical advantage of these precepts.

1. Let the proposed equation be

$$x^3 - 5x^2 + 8x - 1 = 0;$$

then the functions X, X_1 are,

$$\begin{aligned} X &= x^3 - 5x^2 + 8x - 1 \\ X_1 &= \quad 3x^2 - 10x + 8, \end{aligned}$$

and consequently $X_2 = \qquad 2x - 31.$

Here, by help of the foregoing rule, the function X_2 is obtained immediately from the two preceding, independently of any bye operations: thus $2x$ is found by multiplying $8x$ by twice 3, or 6, and subtracting -5 times $-10x$ from the result, changing the sign of the remainder; -31 is obtained by multiplying -1 by three times 3, or 9, and subtracting -5 times 8 from the result, changing the sign of the remainder, as before.

2. Let the equation be

$$x^4 - 2x^3 - 7x^2 + 10x + 10 = 0;$$

then we have

$$\begin{aligned} X &= x^4 - 2x^3 - 7x^2 + 10x + 10 \\ X_1 &= \quad 2x^3 - 3x^2 - 7x + 5 \end{aligned}$$

and consequently

$$X_2 = \qquad 17x^2 - 23x - 45.$$

In this example the coefficients -2 and 2 are rendered equal, and of the same sign as the latter, by means of the factors -1 and 1; employing these, therefore, instead of the coefficients themselves as multipliers, we obtain $17x^2$ by multiplying $-7x^2$ by twice 1 or 2, and subtracting -1 times $3x^2$, changing the sign of the remainder; the next term is derived from multiplying $10x$ by three times 1, or 3, subtracting -1 times $-7x$ from the result, and changing the sign; and the last term is got by multiplying 10 by four times 1, or 4, subtracting -1 times 5, and changing sign.

3. Lastly, let the equation

$$2x^4 - 13x^2 + 10x - 19 = 0$$

be proposed, in which the second term is zero. Then we have

$$\begin{aligned} X &= 2x^4 + 0x^3 - 13x^2 + 10x - 19 \\ X_1 &= \quad 4x^3 + 0x^2 - 13x + 5 \end{aligned}$$

∴ by precept (B.)

$$X_2 = \qquad 26x^2 - 30x + 76$$

or

$$13x^2 - 15x + 38.$$

The principles upon which the foregoing precepts depend may be explained in a few words. As the coefficients in X_1 are severally obtained by multiplying the corresponding coefficients in X, each by the exponent in the term to which it belongs, the derived coefficients are regularly decreasing multiples of the original coefficients, the first in X_1 being n times the first in X; the second in X_1, $n-1$ times the second in X, and so on. Now in order to effect the division of X by X_1, we must render the leading coefficients divisible; and for this purpose we multiply all the terms of X by n, and thus make the leading coefficient in X the same as that in the divisor X_1; so that the first term of the quotient being simply x, it is obvious that the first remainder will always consist of the second term of X, twice the third term, three times the fourth term, and so on. To render the leading term of this remainder divisible by that of the same divisor X_1, we multiply the entire remainder by the first coefficient in that divisor, and we thus get a result which, from what has just been said, is the same as would arise from multiplying the second term of X by the first coefficient in X_2, the third term by twice that coefficient, the fourth by three times the same coefficient, and so on: the second remainder, that is $-X_2$, is evidently obtained by subtracting from this result the product of X_1 by the second coefficient of X; and as this subtraction necessarily destroys the term in the former remainder, neither that term nor the one which annuls it need to have been written down. From these considerations the rule (A.) immediately flows; the precept (B.) is an obvious deduction from it; and the note is only such a modification of it as is necessary to avoid the introduction of common factors into the terms of X_2.

There are means also of abridging the process for determining the other functions X_3, X_4, &c.; and as the method of Sturm is destined to supersede every other hitherto employed for ascertaining the nature and situation of the roots of an equation, such abridgements of labour are well worth attending to: I may possibly advert to them at a future opportunity.

October 12, 1835. J. R. YOUNG.

LVIII. *On* Bernoulli's *Theory of the Tides. By* J. W. LUBBOCK, *Esq. V.P. and Treas. R.S.**

IT has been shown that the semimenstrual inequality (which is by far the most considerable) in the time and height

* Communicated by the Author.

of high water coincides with the law assigned to it by Bernoulli so nearly, that in this respect Bernoulli's theory and observation may be considered as leading to identical results. The hypothesis of the moving fluid spheroid, however violent it may appear, which ensues as a necessary consequence from the fundamental differential equations which regulate the motions of fluids, when certain quantities are neglected, may therefore securely be admitted as a first approximation. The inequalities due to changes in the moon's parallax and declination are much more minute, and it is more difficult to detect the law which they follow empirically. The discussions which Mr. Dessiou has effected of the London and the Liverpool observations have been made with reference to the moon's transit preceding the time of high water, which seems to me the most convenient course for the object we had immediately in view, namely, to obtain tables to serve for the purpose of predicting the phænomena. But it is probable that the tide-wave, in as much as it results from the action of the sun and moon, may be considered as developed in the main ocean, and as transmitted through the seas which encompass England, modified solely by the wind, and by the resistance of various kinds which it encounters in its passage. Hence, as Bernoulli remarks, the phænomena on our coasts are referable to the places of the luminaries some time previously; and owing to the inequalities in the moon's motion, the moon's parallax and declination corrections depend in some measure upon the transit employed. For since the time of the moon's synodic revolution is 29·53 days, the mean interval of time between two successive transits is $24^{h}\ 49^{m}$: if, therefore, I had taken the transit of the day previous, instead of that immediately previous to the high water, all my intervals with par. $57'$ in the tables published in the Philosophical Transactions would have been increased by $24^{h}\ 49^{m}$, and the argument of the table (moon's transit) would have been diminished by 49^{m}. The angular velocity of the moon being nearly as the square of the parallax, the mean interval between her transits with par. $54'$ is $24^{h}\ 43^{m}$ only; all my intervals for par. $54'$ would therefore have been increased by $24^{h}\ 43^{m}$ only. If I had taken the transit last but two, all my intervals corresponding to par. $57'$ would have been increased by $37^{h}\ 13^{m}$, the argument of the tables would have been diminished by $1^{h}\ 13^{m}$, and I should have found the *fundamental hour* for the port of Liverpool 2^{m} only, instead of $1^{h}\ 15^{m}$. Moreover, all my intervals corresponding to $54'$ parallax would have been increased by $37^{h}\ 4^{m}$ only.

$$37^{h}\ 13^{m} - 37^{h}\ 4^{m} = 9^{m}.$$

The constant portion of the moon's parallax correction, which for Liverpool is $+7^{m}\cdot 9$ for par. 54′, which I formerly noticed as inconsistent with Bernoulli's table, is thus probably accounted for; and we see why this quantity should be rather greater for London, when the argument of the moon's parallax correction in the time of high water is the moon's transit immediately preceding the tide, as in those tables which I have given. When this last consideration is attended to, I have ascertained that the theory of Bernoulli is not less confirmed by the moon's parallax correction as deduced from the discussions of the Liverpool and the London observations than by the semimenstrual inequality. Similar reasoning might be applied to the correction arising from changes in the declination of the luminaries; but as results immediately deduced from the Nautical Almanac would be more conclusive and satisfactory than indirect inferences, I purpose to recur to this view of the question and carefully to compare the results with the expressions which are derived from Bernoulli's well-known hypothesis.

The irregularities which even results deduced from the mean of an immense number of observations present, render minute differences obscure. In order, therefore, to obtain the concurrence of as many observations as possible to determine the law of the inequality, I have adopted the following plan, which seems to me the least objectionable.

Let δP be the difference of parallax, or

The parallax $-57'$.

I suppose the correction to be proportional to δP; hence the correction for parallax $54' =$ three times the correction for parallax 56′, and the total of the absolute corrections for parallaxes $54', 55', 56', 58', 59', 60', 61' = \frac{16}{3}$, the correction for parallax 54′. Whatever the law of the correction may be, it certainly may be considered as proceeding according to powers of δP, and the preceding hypothesis amounts to neglecting all the powers except the first.

I now employ only the total of the corrections deduced from the discussions, and I multiply it by $\frac{3}{16}$, or the equivalent multiplier, in order to have the correction for 54′. The following table exhibits the results, together with the in-

terval, and height for parallax 57′, which may be considered as the semimenstrual inequality.

☽'s Transit.	54′				57′			
	London.		Liverpool.		London.		Liverpool.	
h m	m	f	m	f	h m	f	h m	f
30	+10·5*	—·42*	+ 6·9	—1·08*	1 51·9	22·65	11 18·1	17·53
1 30	+ 8·7	—·51	+ 6·5	—1·35	1 35·9	22·76	11 2·6	17·63
2 30	+ 5·7	—·66	+ 4·3	—1·41	1 18·3	22·73	10 45·7	17·04
3 30	+ 5·4	—·72	+ 2·2	—1·52	1 4·6	22·42	10 33·5	16·17
4 30	+ 4·2	—·84	+ 1·7	—1·82	50·2	21·69	10 24·7	14·89
5 30	+ 1·6	—·89	+ 2·0	—1·38	43·7	21·08	10 23·3	13·39
6 30	+ 4·6	—·72	+ 6·6	—1·50	44·0	19·94	10 40·8	12·53
7 30	+10·1	—·66	+12·9	— ·99	1 2·4	19·46	11 15·7	12·33
8 30	+11·7	—·39	+16·3	— ·79	1 41·4	19·64	11 43·9	13·36
9 30	+17·6	—·39	+13·2	— ·57	2 6·4	20·71	11 51·5	14·79
10 30	+14·4	—·39	+11·1	— ·81	2 11·0	21·33	11 46·6	16·04
11 30	+12·3	—·45	+ 9·5	— ·93	2 3·3	22·12	11 32·2	17·00
Mean	+8·9		+7·9					

In order to obtain the correction for declination from the concurrence of as many observations as possible, I proceeded nearly in the same manner as for parallax; the following table is intended to give the correction in time and height when the moon is in the equator, founded upon the totality of the observations corresponding to a declination less than 15° north or south.

☽'s Transit.	London.	Liverpool.	London.	Liverpool.
	m	m	f	f
30	+10·1	+ 4·1		+ ·47
1 30	+ 4·2	+ 5·5		+ ·63
2 30	+ 8·3	+ 6·7		+ ·64
3 30	+12·8	+ 9·0		+ ·48
4 30	+15·8	+ 9·2		+ ·37
5 30	+20·9	+11·1		+ ·39
6 30	+19·8	+13·0	+ ·40	+ ·65
7 30	+25·1	+ 7·5	+ ·46	+ ·78
8 30	+13·2	+ 5·1	+ ·29	+ ·61
9 30	+16·0	+ 4·2	+ ·47	+ ·43
10 30	+10·1	+ 2·7	+ ·53	+ ·27
11 30	+10·4	+ 3·7	+ ·75	+ ·70
Mean	13·9	6·8		

* The figures in the columns marked with an asterisk cannot be liable to any error of consequence, as they each may be considered as resulting from about 800 observations. Those of the semimenstrual inequality are each deduced from the mean of from 100 to 150 observations.

Where the blank is left, the London results are too irregular to be entitled to any dependence. The London correction in time is much greater than that for Liverpool.

If x, y, z are the coordinates at the end of the time t, of any element $\mathrm{d}M$ of the ocean; ρ the density of the fluid; $X\,\mathrm{d}M$, $Y\,\mathrm{d}M$, $Z\,\mathrm{d}M$ the components parallel to the coordinate axes of the force acting upon $\mathrm{d}M$; and if the components of the velocity of the element $\mathrm{d}M$, in two positions, which it occupies successively, are u, v, w, and $u + u'\,\mathrm{d}t$, $v + v'\,\mathrm{d}t$, $w + w'\,\mathrm{d}t$; the differential equation to the surface of the ocean will be

$$(X - u')\,\mathrm{d}x + (Y - v')\,\mathrm{d}y + (Z - w')\,\mathrm{d}z = 0.$$

(See M. Poisson's *Traité de Mécanique*, vol. ii. p. 669.)

If the forces arise from attractions or repulsions directed towards fixed or moveable points,

$$X\,\mathrm{d}x + Y\,\mathrm{d}y + Z\,\mathrm{d}z = \mathrm{d}V.$$

This condition obtains in the forces which produce the tides.

$$u' = \frac{\mathrm{d}u}{\mathrm{d}t} + u\frac{\mathrm{d}u}{\mathrm{d}x} + v\frac{\mathrm{d}u}{\mathrm{d}y} + w\frac{\mathrm{d}u}{\mathrm{d}z}.$$

Generally, if $X\mathrm{d}x + Y\mathrm{d}y + Z\mathrm{d}z$ is the exact differential of any function V with reference to the variables x, y, z, and if $u'\,\mathrm{d}x + v'\mathrm{d}y + w'\mathrm{d}z$ may be neglected, the surface of the fluid is given by the equation

$$V = \text{constant.}$$

That is, the surface of the fluid assumes the same form at any given instant, as it would do if the forces then acting upon each particle were invariable in magnitude and direction. It seems worthy inquiry in what cases this approximation is admissible.

If r be the distance of the sun's centre from that of the earth, ζ the sun's zenith distance, m the mass of the sun; if the same quantities accented refer to the moon; and if $\frac{M}{R^2}$ is the force of gravity, R being the distance of the fluid element $\mathrm{d}M$ from the earth's centre; then in the problem of the tides,

$$V = \frac{M}{R} - m\left\{\frac{R\cos\zeta}{r^2} - \frac{1}{\{R^2 - 2rR\cos\zeta + r^2\}^{\frac{1}{2}}}\right\}$$
$$- m'\left\{\frac{R\cos\zeta'}{r'^2} - \frac{1}{\{R^2 - 2r'R\cos\zeta' + r'^2\}^{\frac{1}{2}}}\right\}$$

$$= \frac{M}{R} + \frac{m}{r} - \frac{m R^2}{2 r^3} (1 - 3 \cos^2 \zeta) - \frac{m R^3}{2 r^4} \left\{ 3 \cos \zeta - 5 \cos \zeta^3 \right\}$$

$$+ \frac{m'}{r} - \frac{m' R^2}{2 r'^3} (1 - 3 \cos^2 \zeta') - \frac{m' R^3}{2 r'^4} \left\{ 3 \cos \zeta' - 5 \cos \zeta'^3 \right\}$$

If α denote right ascension, δ declination, ϕ geographical latitude, and μ sidereal time,

$$\cos \zeta = \cos \delta \cos \phi \cos (\mu - \alpha) + \sin \delta \sin \phi.$$

$\cos^2 \zeta$ contains the term $\dfrac{\cos^2 \delta \cos^2 \phi}{2} \cos (2 \mu - 2 \alpha)$.

Hence, neglecting constant terms and those of the argument $\mu - \alpha$, &c., the height of high water

$$= D + \frac{3 m R^3}{4 M r^3} \cos^2 \delta \cos^2 \phi \cos (2 \mu - 2 \alpha)$$

$$+ \frac{3 m' R^3}{4 M r'^3} \cos^2 \delta' \cos^2 \phi \cos (2 \mu - 2 \alpha')$$

$$= D + E \{ A \cos (2 \mu - 2 \alpha) + \cos (2 \mu - 2 \alpha') \},$$

where D is a constant depending only on the zero line from which the heights are reckoned.

$$A = \frac{m \cos^2 \delta P^3}{m' \cos^2 \delta' P'^3}, \qquad E = C m' \cos^2 \delta' P'^3,$$

P being the horizontal parallax, and C a constant depending upon geographical latitude.

By differentiating the expression for the height, in order to find when the height is a maximum, the following well-known formula is obtained:

$$\tan (2 \mu - 2 \alpha') = \frac{A \sin (2 \alpha' - 2 \alpha)}{1 + A \cos (2 \alpha' - 2 \alpha)}.$$

The readiest method of calculating tables of the inequalities of the heights and intervals from the above, which coincide with Bernoulli's expressions, is to obtain the angle $\psi = \mu - \alpha'$, from the expression

$$\tan 2 \psi = \frac{A \sin 2 \phi}{1 + A \cos 2 \phi},$$

for given values of ϕ, then the height of high water

$$= D + E \{ \cos 2 \psi + A \cos (2 \phi - 2 \psi) \}.$$

The value which I formerly deduced from the London observations for the constant A with parallax $57'$, and when $\delta = \delta'$, (see Phil. Trans. 1831, p. 387,) is $\cdot 3788$; log.

$A = 9{\cdot}5784858$. This constant at Liverpool appears to have precisely the same value. I doubt whether much stress ought to be laid upon any slight difference in the semimenstrual inequality if deduced only from a few observations. As this constant is the same for Liverpool and for London, I am particularly anxious to ascertain from the Brest observations, which I have not hitherto succeeded in obtaining, whether it is the same for that port.

I make for the Liverpool old docks, with parallax 57′ and when $\delta = \delta'$,

$$D = \overset{\text{Ft.}}{8} \qquad\qquad E = 6{\cdot}969$$

and for the London docks, from the sill of the Dock-gates,

$$D = \overset{\text{Ft.}}{16{\cdot}68} \qquad\qquad E = 4{\cdot}448.$$

Bernoulli's theory amounts to supposing the surface of the ocean the same as if it were given by the equation

$$\mathrm{X}\,\mathrm{d}x + \mathrm{Y}\,\mathrm{d}y + \mathrm{Z}\,\mathrm{d}z = 0,$$

and therefore to neglecting the quantity

$$u'\mathrm{d}x + v'\mathrm{d}y + w'\mathrm{d}z,$$

which seems to require either that u', v', and w' are small and negligible in comparison with X, Y, Z, or that the quantity $u'\mathrm{d}x + v'\mathrm{d}y + w'\mathrm{d}z$ is separately equal to zero. In order, therefore, to prove *à priori* the justice of Bernoulli's hypothesis it would be sufficient to estimate the value of these quantities.

Euler's method of considering the problem is the same in substance as that of Bernoulli.

LIX. *On the Coralline Crag of Ramsholt and Orford.* By Robert Fitch, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

It was with some surprise that I read Mr. Samuel Woodward's observations, in your last number, respecting a tertiary deposit discovered between the Crag and the London clay, on the banks of the Deben, near Woodbridge, and which has lately been brought into notice by a paper by Mr. Edward Charlesworth inserted in the Lond. and Edinb. Phil. Mag. and Journal for August.

It appears that Mr. Woodward objects to the term "coralline crag" as applicable to the stratum at Ramsholt, and does not agree with Mr. Charlesworth in considering that the beds

in that locality have any connexion with those of Orford and Aldborough.

Although much interested in the organic remains of the crag, I have not until very recently had any opportunity of becoming acquainted with that part of the formation which is now the subject of discussion. During, however, the latter part of this summer, while on a visit at Ipswich, I went over to Ramsholt for the purpose of collecting fossils from that singularly interesting spot. Besides a great variety of shells, I found several species of coral, so abundant, that in the course of a few hours, I obtained from the stratum itself, and the beach below it, more specimens than I could carry away without assistance. I have since compared some of these corals with those from the other localities described by Mr. Charlesworth, and they appear precisely to correspond*. In the hope that the further investigation of these beds, already attended with such highly important results, may be continued,

I have the honour to be, Gentlemen,

Your obedient Servant,

Norwich, Nov. 7, 1835. ROBERT FITCH.

LX. *Reply to* Mr. Woodward's *Remarks on the Coralline Crag; with Observations on certain Errors which may affect the determination of the Age of Tertiary Deposits. By* EDWARD CHARLESWORTH, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

OUR acquaintance with the tertiary strata of this country is so extremely limited, considering the great importance now attached to that particular branch of geological inquiry, that I cannot but feel gratified at finding my endeavours to bring into notice the deposits on our eastern coasts seconded by Mr. Woodward, although his views upon the subject may not exactly coincide with those of my own.

Mr. Woodward, having visited Aldborough and Ramsholt, brings forward some very important objections with refernece to the observations made by me, in your number for August, upon the fossiliferous beds in those localities. To prevent misconception I quote from both communications those passages which bear immediately upon the points at issue.

* The corals which I found at Ramsholt, and a collection made at Orford, are now in my possession; I shall have much pleasure in submitting them to the inspection of any person who is interested in the subject.

Mr. Woodward says, p. 353, "In the first place, his red crag is decidedly diluvium or disrupted crag." As the antagonist to this the following observation occurs in my own remarks: "The red crag affords decisive evidence of having been a gradual deposit, formed by successive accumulations of marine exuviæ, which were not brought from a distant part by the operation of a powerful current, but belonged to the natural inhabitants of those localities, which, owing to the subsequent retreat of the ocean, are now rendered accessible." p. 90.

I was certainly a little startled at seeing it asserted that the shelly strata which I have designated by the term red crag, and which constitute by far the greater part of the crag formation, are of diluvial origin. Mr. Lyell has pointed out the analogy between these deposits and those which are now forming round some parts of the British coast. Professor Phillips regards the crag as an ancient beach of the German Ocean. Messrs. Conybeare and W. Phillips, in describing the crag as a part of the *upper marine formation*, particularly advert to the fact of certain species of *Testacea* occurring naturally grouped together.

Now, I would not for a moment infer, because Messrs. Lyell, Conybeare, and other geological inquirers generally regarded as fully capable of distinguishing between diluvial and regularly formed deposits, have considered the crag as belonging to the latter class, that Mr. Woodward is not in the possession of certain facts which fully justify him in drawing an opposite conclusion, but I regret that he should have thought it sufficient simply to assert that the red crag is of diluvial origin, without making public some of the grounds by which he has arrived at a decision so completely at variance with the facts brought forward by others. If Mr. Woodward had produced something like a reasonable chain of evidence to support his assertion, it would have been more in accordance with the methods usually pursued in determining doubtful points, and would certainly have done far more towards "*eliciting the truth*" than the plan which he has for the present adopted.

Mr. Woodward evidently uses the term 'diluvial' in its most *sweeping* acceptation, as I particularly dwelt upon the fact of the coralline stratum being broken up, and its contents mingled with the newer deposit.

Mr. Woodward's next objection is of a more serious nature. "Secondly, his term 'coralline crag' is not appropriate, as it leads us to suppose that it is composed of corallines, when, in fact, there are none in the Ramsholt bed, which is chiefly adverted to."

In applying the term 'coralline crag' to the Ramsholt de-

posit, I dwelt most strongly upon the analogy existing between the organic remains of that locality and those of Orford and Aldborough. This analogy Mr. Woodward virtually denies, asserting in the most unqualified manner that the term 'coralline crag' is not applicable to Ramsholt, as the Aldborough corallines do not occur there.

For several years I have had constant access to the coralline crag district, and have devoted no inconsiderable portion of time and labour to the investigation of its organic productions. Mr. Woodward resides at Norwich, a distance of sixty miles, and has on one occasion spent a few hours at the locality in question. It is unnecessary for me to expatiate on the breach of decorum committed by Mr. Woodward in making assertions which imply that to support a preconceived opinion with regard to the geological position of the Ramsholt stratum, I have made a wilful misstatement respecting its organic remains. The generality of persons interested in geological pursuits are quite capable of estimating how far a single visit to the coralline crag would give Mr. Woodward such an insight into its organic productions as would justify the statement which he has made on the present occasion. He proceeds:

"Mr. Charlesworth has not mentioned the great coral-reef situate about three quarters of a mile north of Aldborough, which contains (as far as I have observed) neither univalves nor bivalves, except a few Pectens*."

Mr. Woodward's examination of my paper must have been as cursory as his acquaintance with the subject upon which it treats appears to be superficial. In this particular instance you, Gentlemen, have pointed out his inaccuracy, as shown by the following extract from my own observations.

"One or two writers in alluding to the crag have observed, that it assumes a remarkable change in character at Aldborough, the eastern boundary of the formation. Mr R. C. Taylor particularly remarks this circumstance, describing this part of the stratum as a 'soft porous rock mixed with interesting varieties of coral and sponges'." I then proceed to point out the analogy which the crag of that neighbourhood bears to coral-reefs.

If Mr. Woodward were to determine what organic remains may be found at Lyme Regis by viewing the cliffs from the beach, he would undoubtedly come away with the full

* Mr. Woodward states that the coral-reef is three quarters of a mile north of Aldborough, because a quarry happens to have been opened there. Mr. Woodward is evidently not aware that the crag of the whole neighbourhood is of the same description.

conviction that the lias in that part of the coast contains neither *Ichthyosauri* nor *Plesiosauri*, but only a few Ammonites. Had he made use of a *pickaxe* in his examination of the coral-reef at Aldborough, he would never have hinted that its *Testacea* were limited to a few Pectens. I have in vain endeavoured to comprehend what Mr. Woodward can mean by saying, as he next does, that when I included Ramsholt as part of the coralline crag formation, I must have had the coral-reef at Aldborough in my mind's eye.

In Mr. Woodward's work on the Geology of Norfolk, he speaks of a bed of shells analogous to the *calcaire grossier* of Paris having been discovered "*beneath the Suffolk crag*" at Ramsholt. It now appears that this was stated on the authority of another person, and that Mr. Woodward having himself examined the Ramsholt stratum, finds it to be only a bed of undisturbed crag, covered by transported fragments derived from the same deposit. The inconsistency in the two accounts is certainly remarkable. It should not, however, be forgotten that Mr. Woodward formed his opinion from a single visit, while it is more than probable that his friend drew his conclusion from an intimate acquaintance with the locality in question. Surely Mr. W. would never have inserted so important a paragraph in his work, unless he felt confidence in the judgement of the person by whom it was communicated.

Having determined, without assigning a shadow of a reason for so doing, that the red crag is a diluvial deposit; having impugned my veracity on, perhaps, the most important statement brought forward in my paper; having also visited the coralline crag at Aldborough, and affirmed that it contains neither univalves nor bivalves, except a few Pectens; and in addition to this, having pronounced the Ramsholt stratum to be merely a bed of undisturbed crag, covered by transported materials of the same deposit, my commentator next intimates that he has not the least wish "*to underrate the merits of my valuable communication*"!

I think it probable that the connexion existing between the coralline crag and the overlying fossiliferous deposits will prove a more interesting subject for investigation, and give rise to more important suggestions than might at first sight be imagined. The possibility that a considerable number of *Testacea* peculiar to one deposit may have been introduced into another of more recent formation, without our being in possession of any clue to ascertain the extent to which such an admixture has taken place, is a question of the deepest importance, as modifying the views generally adopted regarding the chronological arrangement of tertiary strata.

Had the various species of *Terebratula* and other chalk fossils, which are of such frequent occurrence in the crag of Norfolk, been derived from a tertiary instead of a secondary formation, their extraneous origin would not have been detected, and they would have been regarded as among those shells which are common to different tertiary periods. In determining the age of the Norfolk crag according to the principles laid down by Mr. Lyell, these shells would have swelled the list of extinct species, and the important fallacy which must thence have arisen is too obvious to require pointing out. I shall shortly make public some observations on the subject of fossils belonging to different periods being occasionally associated in the same deposit. I have strong evidence to show, that, in the same way as in Norfolk secondary fossils have been introduced into the overlying tertiary beds,—in the same way as in Suffolk I believe the *Testacea* of the coralline crag to have been removed into the superincumbent deposit,—so also are the formations now in progress along some parts of our eastern coast deriving no inconsiderable proportion of their organic contents from the destruction of strata which are supposed to represent the organization of the *older pliocene period*. If we may look forward to the time when these deposits shall have become the subject of geological inquiry, and suppose that their age is to be determined by comparing such fossil shells as may be obtained from them with the then existing species, how evidently inaccurate will be the conclusions formed from such an examination! By the time that the deposits now in progress become accessible, every vestige of the crag will probably have disappeared. Hence there will be nothing to excite the slightest suspicion that they contain the organic products of different periods, any more than there would have been in the instance of the red crag had the coralline been entirely destroyed *.

* These observations have no reference to the principles which Mr. Lyell has advocated in determining the age of tertiary deposits: they apply exclusively to the errors that may arise in the application of those principles. The bed of the German Ocean, between Scarborough and Herne Bay, is strewed with the bones of extinct Mammalia, in prodigious numbers, and to which abundance of *living* Balani, with other Testacea, are adherent. There is, perhaps, no limit to the complication of errors which have arisen from its being universally considered that the association of different organic remains in a regularly stratified deposit necessarily implied their co-existence. In illustration of this I would refer to a paper by the Rev. Mr. Vernon (now Vernon Harcourt) in the Philosophical Magazine for September 1829 (Phil. Mag. and Annals, N. S. vol. vi. p. 225 *et seq.*) on the discovery of extinct Mammalia in a deposit with recent species of Testacea at North Cliff. See also Mr. Lyell's observations on the same subject, in his "Principles of Geology."

I should not have entered upon this subject at present had not Mr. Lyell, in the new edition of his Principles of Geology, mentioned, that the fact of 150 species being common to the upper and lower divisions of the crag, was a proof of their belonging to the same period, without alluding to the occurrence of the secondary shells in the crag of Norfolk. Should the red and the coralline crag be ultimately referred to the same age, it will not in the least affect the question now brought forward.

On again perusing Mr. Woodward's *critique* on my examination of the tertiary beds in Suffolk, I cannot avoid remarking that I think it would have been more judicious in him to have postponed its publication until he had acquired a more extensive acquaintance with those localities to which I have particularly directed my attention. From his confessedly slight acquaintance with the Suffolk crag, he must be incompetent to support the assertions he has made; and some of the objections he has advanced against my views can only be regarded as frivolous and vexatious. Mr. Woodward's name has been longer associated with the crag than my own, and the remarks he has made relative to the statements contained in my paper are calculated to produce a most unfavourable impression, not merely as regards my discrimination on the subject in question, but as to the actual reliance which may be placed upon my testimony. Whether any theoretical views I entertain may be confirmed or disproved by others, is a matter of little consequence, compared with the imputation that my evidence, as to matters of fact, is unworthy of credence.

The coralline crag is a deposit so rich in fossils, and at the present time in many respects so peculiarly interesting, that it must sooner or later become an object of general attention, and it will then be shown how far the conclusions I have drawn can be supported by the observations of others.

While however, I am most anxious that every possible opportunity should be embraced of pursuing that investigation which is now set on foot, and while I would have every opinion that may be advanced subjected to the most rigid criticism and the fullest discussion, I must at the same time protest against any individual, whatever may be his geological attainments, visiting the coralline crag, and at one glance deciding that my conclusions are erroneous and the facts misstated. It is very possible that a person might go into the quarry by the side of the road near Aldborough, and, like Mr. Woodward, he might only see a few Pectens, or, perhaps, spend an hour or two at Ramsholt, and not meet with any

of the Aldborough corals; but surely there are few who, from such limited data, would consider themselves qualified to dispute the accuracy of results obtained from the more extensive investigation of others. I could bring forward considerable evidence (in addition to that which has already appeared) for the purpose of showing that the red crag is not of diluvial origin, and that the Ramsholt deposit is a part of the coralline stratum. It appears to me, however, that the reliance Mr. Woodward places on the testimony of others does not extend beyond such facts as fall within the sphere of his own observations also: any additional facts that I might adduce in support of my previous views, being new to him, might therefore be regarded as fabrications.

W. M. Higgins, Esq., F.G.S., has in his possession a manuscript paper by Mr. R. C. Taylor, in which is pointed out the separation between the lower and upper beds of crag, and to which I shall more particularly allude in a future communication.

I am, Gentlemen, yours, &c.

Guy's Hospital, Nov. 16, 1835. EDW. CHARLESWORTH.

LXI. *Experimental Investigation of a Formula for inferring the Dew-point from the Indications of the Wet-bulb Hygrometer. By* JAMES APJOHN, *M.D., Professor of Chemistry in the Royal College of Surgeons, Ireland.*

[Continued from p. 274, and concluded.]

THE most direct method of testing our formula consists, as has been already observed, in comparing its results with dew-points experimentally determined. In order, however, that this criterion be decisive, it is not only necessary that the depressions be considerable in amount, but also, as is obvious, that the dew-points be accurately known. Now, the registers to which I have had access do not perfectly satisfy either of these conditions, the depressions being generally small, and the observations made with an instrument, Daniell's hygrometer, the difficulty of observing with which is universally admitted. In reflecting on this matter it occurred to me that both difficulties might be evaded in the following simple manner. Let air saturated with moisture, and whose temperature is, therefore, necessarily its dew-point, be heated, and let the temperature of the heated air be taken, as also that shown by a moist-bulb hygrometer subjected to the action of a current of it. Let then, by the application of the formula, the dew-point belonging to the two latter observa-

tions be calculated, and, from a comparison of it with the original temperature of the air when saturated with humidity, we shall be enabled to pronounce with confidence upon the value of our method.

In the experiments which I performed on this plan, the air was saturated with moisture by forcing it from a bellows through a succession of four Woulfe's bottles, connected in the usual way, so as to cause the air to pass in each bottle through about two inches of water, and the air thus saturated was heated by being made to pass through a coil of copper tubing immersed in a tub of warm water, the thermometer and hygrometer being placed with their bulbs within a quarter of an inch of each other in a narrow glass tube attached to the further extremity of the copper worm. The following are the results thus obtained:

	t	t'	d	p	t'' obs.	t'' calc.	Diff.
1835. April 17, 11 A.M.	78	62·2	15·8	30·30	51·3	50·47	— ·83
	76	61·5	14·5	30·30	51·3	50·26	—1·04
	73	60·3	12·7	30·30	51·3	51·58	+ ·28
	72	60	12	30·30	51·3	50·81	— ·49
	69	58·6	10·4	30·30	51·3	50·40	— ·90
April 18, 11 A.M.	90·5	67	23·5	30·15	50·8	50·17	— ·63
	82·2	64·3	17·9	30·15	50·9	51·	— ·10
	79	62	16·4	30·15	50·9	50·23	— ·67
	71·7	60	11·7	30·15	51·2	50·66	— ·54
	69	58·9	10·1	30·15	51·5	50·70	— ·80
April 20, 11 A.M.	92	69	23	30·42	54·1	54·40	+ ·30
	83	65·8	17·2	30·42	54·5	54·36	— ·14
	76	63·3	12·7	30·42	54·7	54·54	— ·36
	68	60·3	7·7	30·42	55·	54·74	— ·26
April 21, 11 A.M.	98·5	71·5	27	30·36	55·5	55·51	+ ·01
	84·6	67	17·6	30·36	56·	55·79	— ·21
	77·5	64·5	13	30·36	56·3	55·97	— ·33
	81	62·2	8·8	30·36	56·5	56·18	— ·32
April 22, 11 A.M.	83	66·5	16·5	30·51	56·8	55·87	— ·93
	77	65	12	30·51	57·2	57·23	+ ·03
	71·3	63	8·3	30·51	57·5	57·47	— ·03
April 23, 11 A.M.	91·8	68·6	23·2	30·51	54·1	53·70	— ·40
	75·2	63·2	12	30·51	55·	54·94	— ·06
	72	62	10	30·51	55·1	54·98	— ·12
						Mean	— ·35

By a glance at the preceding table, which includes twenty-four distinct observations, we shall perceive, 1st, That in the case of seven of them the observed and calculated dew-points are almost coincident; 2nd, That the difference in no instance exceeds, and in but a single instance reaches, one degree; and 3rd, That the mean difference deducible from the whole is

but ·35, or about one third of a degree Fahrenheit. It will also be noted that the difference is negative, or that the mean calculated dew-point is lower than the observed, and not *vice versâ.* If we were justified in considering this latter result as anything more than accidental, it might certainly be urged as an argument against the strict accuracy either of our experiments or of our theoretical views; for the corrections for the influence of pressure and aqueous vapour on the specific heat of air being neglected in the preceding calculations, the calculated dew-points instead of being lower should be higher than the truth. In order, in fact, to account for the discrepancy in question, supposing it to be well established, it would be necessary to conclude either that m, the coefficient of our hygrometric formula, is assumed somewhat too great, or that the observed depressions are a little too small. The first I believe to be the true solution, and I am at present disposed to consider m as more correctly represented by the fraction $\frac{1}{88}$ than $\frac{1}{87}$. This point, however, I have not as yet been able fully to satisfy myself upon, nor can the more exact determination of the value of the constant be considered a matter of much practical importance, since the formula in its present state conducts, as we have seen, to results which harmonize admirably with each other and with observation.

I shall conclude by subjoining a couple of tables, by the aid of which the application of my formula $f'' = f' - \frac{d}{87} \times \frac{p}{30}$ to the determination of the dew-point is greatly facilitated. Table A, which I have taken from the Edinburgh Encyclopædia, article Hygrometry, gives the elastic force of the vapour of water for every degree Fahrenheit between 0° and 100° inclusive. Table B gives $\frac{d}{87 \times 30}$ for every value of d between ·1 and 10. This quotient, as is obvious from a glance at the formula, is, in calculating an observation, to be multiplied by p, the existing pressure, and the product when deducted from f', as given by table A, will afford f'', or the tension of vapour at the dew-point. Should the depression exceed 10°, the value of $\frac{d}{87 \times 30}$ may still be got from table B by addition. Thus, if $d = 13°$, $\frac{d}{87 \times 30} = ·00383 + ·00114 = ·00497$.

A.

t	f	t	f	t	f	t	f	t	f
0	·06121	21	·13408	41	·27376	61	·54089	81	1·03350
1	·06359	22	·13906	42	·28346	62	·55913	82	1·06656
2	·06605	23	·14421	43	·29348	63	·57795	83	1·10058
3	·06861	24	·14954	44	·30384	64	·59735	84	1·13559
4	·07126	25	·15506	45	·31453	65	·61734	85	1·17161
5	·07401	26	·16076	46	·32557	66	·63795	86	1·20867
6	·07685	27	·16667	47	·33684	67	·65919	87	1·24680
7	·07980	28	·17277	48	·34875	68	·63108	88	1·28602
8	·08286	29	·17908	49	·36090	69	·70364	89	1·32636
9	·08603	30	·18561	50	·37345	70	·72688	90	1·36785
10	·08931	31	·19237	51	·38640	71	·75083	91	1·41059
11	·09270	32	·19934	52	·39977	72	·77551	92	1·45438
12	·09622	33	·20658	53	·41356	73	·80092	93	1·49948
13	·09987	34	·21404	54	·42779	74	·82710	94	1·54585
14	·10364	35	·22175	55	·44249	75	·85407	95	1·59352
15	·10755	36	·22972	56	·45764	76	·88184	96	1·64251
16	·11160	37	·23796	57	·47328	77	·91042	97	1·69286
17	·11579	38	·24647	58	·48940	78	·93987	98	1·74461
18	·12013	39	·25527	59	·50604	79	·97017	99	1·79778
19	·12462	40	·26436	60	·52320	80	1·00137	100	1·85241
20	·12527								

B.

d	$\frac{d}{87\times30}$	d	$\frac{d}{87\times30}$	d	$\frac{d}{87\times30}$	d	$\frac{d}{87\times30}$	d	$\frac{d}{87\times30}$
·1	·00003	2·1	·00080	4·1	·00157	6·1	·00233	8·1	·00310
·2	·00007	2·2	·00084	4·2	·00160	6·2	·00237	8·2	·00313
·3	·00011	2·3	·00087	4·3	·00164	6·3	·00241	8·3	·00317
·4	·00015	2·4	·00091	4·4	·00168	6·4	·00245	8·4	·00321
·5	·00019	2·5	·00095	4·5	·00172	6·5	·00248	8·5	·00325
·6	·00022	2·6	·00099	4·6	·00176	6 6	·00252	8·6	·00329
·7	·00026	2·7	·00103	4·7	·00180	6·7	·00256	8·7	·00333
·8	·00030	2·8	·00107	4·8	·00183	6·8	·00260	8·8	·00337
·9	·00034	2·9	·00111	4·9	·00187	6·9	·00264	8·9	·00340
1	·00038	3	·00114	5	·00191	7	·00268	9	·00344
1·1	·00042	3·1	·00118	5·1	·00195	7·1	·00271	9·1	·00348
1·2	·00045	3·2	·00122	5·2	·00199	7·2	·00275	9·2	·00352
1·3	·00049	3·3	·00126	5·3	·00202	7·3	·00279	9·3	.00356
1·4	·00053	3·4	·00130	5·4	·00206	7·4	·00283	9·4	·00360
1·5	·00057	3·5	·00134	5·5	·00210	7·5	·00287	9·5	·00363
1·6	·00061	3·6	·00137	5·6	·00214	7·6	·00291	9·6	·00367
1·7	·00065	3·7	·00141	5·7	·00218	7·7	·00294	9·7	·00371
1·8	·00068	3·8	·00145	5·8	·00222	7·8	·00298	9·8	·00375
1·9	·00072	3·9	·00149	5·9	·00225	7·9	·00302	9·9	·00379
2	·00076	4	·00153	6	·00229	8	·00306	10	·00383

James Apjohn, M.D.,
Prof. of Chemistry in the Royal College of Surgeons, Ireland.

LXII. *On the Cause of the 'Coloured Bands' observed by* A. R. *By* A Correspondent.

THE coloured bands described by A. R. in the last number of this Journal, p. 363, are exactly the same as those discovered by Sir David Brewster and described in the Edinburgh Philosophical Transactions, vol. vii. page 435. Sir John Herschel, in his Treatise on Light, page 475, 476, has given a minute account of these bands as "affording an excellent illustration of the laws of periodicity observed by the rays of light in their progress, whether, as in the Newtonian doctrine, we consider them as subjected to alternate fits of easy reflection and transmission, or, as in the undulatory hypothesis, as passing through a series of phases of alternately direct and retrograde motions in the particles of æther in whose vibrations they consist."

The bands under consideration are produced entirely by the plates of parallel glass between which A. R. had placed his convex lens, and are dependent upon the inclination of these plates, to the common section of which they are parallel. Since the publication of his memoir Sir David Brewster has observed the same fringes stretching with singular brilliancy across the fourth and sixth images formed by total reflection from the posterior surfaces of two plates of common mirror glass inclosing water.

In the work already referred to, Sir John Herschel has given a perspicuous explanation of these phænomena in their general details; and he has adverted also to another series of coloured fringes coexisting with the first series, which Sir David Brewster describes "as far surpassing in precision of outline and in richness of colouring every analogous phænomenon which he had seen."

"By intercepting," says Sir John, "the principal transmitted beams in the direct image, and receiving only those portions of the rays going to form it whose curves are as in fig. 140, Dr. Brewster succeeded in rendering visible a set of coloured fringes, which in general are diluted and concealed in the overpowering light of the direct beam. They originate, evidently, in the interference of those two rays whose courses are each represented by $4t + 1$, and would therefore be strictly equal if the plates were exactly parallel. Their theory, after what has been said, will be obvious on inspection of the figure, as well as those of all the rest of the systems of fringes described in that highly curious and interesting memoir." (Herschel on Light, p. 476, § 694.)

Nov. 11th, 1835.

LXIII. *On the immediate Transmission of Calorific Rays through Diathermal Bodies.* By M. Macedoine Melloni.*

At the last meeting of the British Association for the Advancement of Science, Mr. H. Hudson and Mr. Powell furnished several communications on radiant caloric †. After having cited some of my experiments on calorific transmission, these ingenious philosophers endeavoured to explain them by hypotheses which in my opinion can no longer be sustained in the present state of science. I wish to direct inquiry to a subject which by its intimate connexion with the fundamental properties of one of the principal agents of nature, appears to me worthy to engage our attention.

For a long time the immediate transmission of terrestrial radiant heat by transparent substances, both solid and liquid, has been denied; and the opinion has become prevalent that we see in experiments of this kind only an effect of the heat absorbed by the body submitted to the calorific radiation. Hence, from the first researches which I undertook upon the immediate transmission of heat, I have endeavoured to render my observations entirely independent of the heating effect proper to the diaphanous plate submitted to experiment; and I succeeded in this by a very simple arrangement, which consists in diminishing as much as possible, in the first instance, the heating effect of the plate, by placing it at a considerable distance from the source, and then in rendering its action upon the thermoscope *wholly insensible*, by removing the instrument to the requisite distance from the plate itself. But in order to experiment under these circumstances, it is clearly necessary to employ an extremely delicate thermoscope, such as well-constructed thermomultipliers otherwise, the feeble rays of heat, direct or transmitted, which arrive from the distance at which the instrument is fixed, would produce no perceptible effect. Further, when any one wishes to make experiments on the transmission of caloric, he may always assure himself that the condition above mentioned is fulfilled. For that I have given four different proofs: the following is the one which is inserted in the Report on Radiant Heat made by M. Biot to the Académie des Sciences; it will soon be seen why I have preferred this proof to the others.

Let us suppose the source of heat, the body, and the thermomultiplier in the proper positions. The plate of the dia-

* Communicated by the Author, through Michael Faraday, Esq., D.C.L., F.R.S.

† Abstracts of these communications have been given in pp. 296–298 of our present volume.—Edit.

thermal substance employed will then be applied against the central opening of the metallic screen: it will immediately transmit a certain quantity of radiant heat, which will penetrate into the cylindrical covering of the pile placed at a distance behind the screen, and directed upon the prolongation of the line drawn from the source to the centre of the opening: the indicating needle of the galvanometer connected with the thermoelectric pile will be set in motion, and will take a greater or a less deviation according to the diathermaneity (*diathermaneïté*) of the substance of which the plate consists. After having noted this arc of deviation, let the pile be removed by degrees from the direction of the immediately transmitted calorific rays, taking care always to hold the opening of its covering turned toward the plate, the distance of which from the pile ought not to vary. We shall then see the deviation of the galvanometer diminish gradually, and be reduced exactly to zero, when the covering of the pile shall have entirely left the conical space occupied by the pencil of emergent heat; which supplies the most complete proof that the heating effect due to the plate itself does not exercise the least perceptible influence on the actual conditions of the apparatus.

To render the force of this demonstration still greater, we may bring the pile several centimetres toward the plate, while we remove it from the immediate direction of the rays. We may also turn the plate upon its vertical axis, and place it opposite the opening of the instrument removed from the calorific cone, without the least deviation being manifested by the galvanometer in either the one case or the other.

It is thus decisively proved by this experiment, that the heat from the source traverses the plate, *preserving its radiant form*; that the calorific rays are propagated beyond the plate *in their original direction only* (*dans le seul sens de leur direction primitive*); and that *all the effect produced*, in the case in which the axis of the pile is in front of the central opening of the screen, is attributable to the action of the radiant heat transmitted immediately by the plate. This mode of demonstration being independent of the nature of the rays, is equally applicable to dark or luminous radiant heat.

Now Mr. Hudson, in removing his thermo-electric pile out of the direction of the calorific rays emitted by a vessel full of hot water, finds that the needle of the galvanometer remains at zero when the opening of the screen is free; but he still observes a very sensible deviation in the case in which the opening is closed with the diaphanous plate. What must we conclude? Evidently, that the circumstances under which

Mr. Hudson experimented were by no means favourable for studying the immediate transmission of radiant caloric through solid bodies; and yet that philosopher cites his results as facts tending to prove that there is no immediate passage of simple heat through that class of bodies. His induction, although presented under a doubtful form, does not appear to me permissible.

Mr. Powell performed in 1825 a very beautiful experiment upon radiant caloric*; it consists in proving that the ratio of calorific absorption of a white surface to that of a black one is not the same for the rays proceeding directly from the source, and for the rays transmitted by a plate of glass. The sources of heat employed by Mr. Powell were an Argand lamp and iron heated to a bright red. I have had occasion more recently to verify this fact, which holds good not only with the glass, but with all diathermal substances, rock-salt excepted. In order to explain this phænomenon, as well as the old experiments of calorific transmission, Mr. Powell admitted that flame and incandescent metals radiate *two kinds* of heat, the *luminous* and the *obscure*, the first of which alone is capable of traversing the glass, whilst the second is entirely absorbed by that substance. He even now thinks that the entire series of my experiments may be explained on this supposition, which he without doubt has modified, in conceding that the interception by solid bodies in general is not a distinctive character of the non-luminous heat, since, in certain cases, it traverses these bodies with the same ease as the most luminous heat. If Mr. Powell alludes to experiments analogous to his own, that is to say, the series of observations which have been made with the pile having one of its faces whitened and the other blacked, I am of his opinion; but I differ from him totally if he admits that the hypothesis of two heats suffices to explain all the facts relative to the transmission. I will limit myself to citing some results which appear to me decisive. If we expose a common plate of glass of one or two millimetres in thickness to the calorific rays of Locatelli's lamp emerging from a black opake glass, then to the immediate radiation of a plate of copper heated to 400° [Cent.?], and finally to the heat emitted from a vessel full of boiling water, we find that its transmission is $\frac{70}{100}$ to $\frac{80}{100}$ of the incident heat in the first case, $\frac{12}{100}$ to $\frac{15}{100}$ in the second, and 0 in the third. Now here the three radiations *consist exclusively of non-luminous heat;* and yet their transmissibility across the same plate is so different, that nearly all the incident rays

[* See Phil. Trans. 1825, or Phil. Mag., First Series, vol. lxv. p. 437 *et seq.*—EDIT.]

of the heat emitted by the lamp pass immediately, while those of the heat emitted by the boiling water are completely absorbed. It is scarcely necessary to add that we should have other transmissions if we took calorific sources of different temperatures from those I have just cited. *There are, then, several kinds of dark heat,* as there undoubtedly exist several kinds also of calorific rays in the heat which ordinarily accompanies light.

Paris, Nov. 15, 1835. MACEDOINE MELLONI.

LXIV. *On the Transformation of Equations. By* G. B. JERRARD, *A.B.*

[Continued from page 203.]

IN order to reduce the general equation of the mth degree

$$x^m + A\,x^{m-1} + Bx^{m-2} \ldots + J\,x^{m-n-1} + K\,x^{m-n} \ldots + V = 0,$$

to the form

$$y^m + K'\,y^{m-n} \ldots + V' = 0,$$

in which $\quad A' = 0, \quad B' = 0, \ \ldots \ J' = 0,$

I saw that it would be necessary when $y = P + Q\,x + R\,x^2 \ldots + L\,x^{\lambda}$, to satisfy $(n-1)$ equations, which relatively to P, Q, R, ... L were of 1, 2, 3, ... $(n-1)$ dimensions, and which in the notation of the Researches* would be expressed by

$$\int . (O\,P + 1\,Q + 2\,R \ldots + \lambda\,L) = 0,$$
$$\int . (O\,P + 1\,Q + 2\,R \ldots + \lambda\,L^2) = 0,$$
$$\int . (O\,P + 1\,Q + 2\,R \ldots + \lambda\,L^3) = 0.$$
$$. \quad . \quad . \quad . \quad . \quad . \quad . \quad .$$
$$\int . (O\,P + 1\,Q + 2\,R \ldots + \lambda\,L)^{n-1} = 0.$$

No general method had as yet been discovered by mathematicians for determining P, Q, R,... without the aid of an equation of 1, 2, 3,... $(n-1)$ dimensions, when the idea occurred to me, that if, availing myself of the indefinite extent of the series for y, I could detach some of the unknown quantities P, Q, R,... from the rest so as to form a succession of groups, I might possibly be enabled to effect the determination of these quantities by means of equations of 1, 2, 3,... $(n-1)$ dimensions only. But here a great difficulty presented itself. For the purpose of detaching an unknown quantity A from a function of n dimensions relatively

* See Jerrard's Mathematical Researches, p. 41.

to A, and the other quantities which were considered as unknown

$$\overset{0}{\mathrm{K}} A^n + \overset{1}{\mathrm{K}} A^{n-1} + \overset{2}{\mathrm{K}} A^{n-2} \ldots + \overset{n}{\mathrm{K}},$$

it was necessary not only to make

$$\overset{1}{\mathrm{K}} = 0, \quad \overset{2}{\mathrm{K}} = 0, \ldots \overset{n}{\mathrm{K}} = 0,$$

where, as is expressed by the indices 1, 2, ... n, the unknown quantities rose to the 1st, 2nd, ... nth degrees respectively, but also to make

$$\overset{0}{\mathrm{K}} = 0,$$

into which none of these quantities, however numerous they might be, would ever enter. By means of previous transformations I could in a few instances succeed in finding as many equations

$$\overset{0}{\mathrm{K}}{}' = 0, \quad \overset{0}{\mathrm{K}}{}'' = 0, \ldots$$

as there were quantities to be detached. But when the number of these was increased beyond a certain very narrow limit, it became a problem of greater difficulty to effect the preparatory transformation than to solve the problem with which I had set out. At length by means of a coalition of certain of the unknown quantities A', A'', A''',... I succeeded in forming a development *all* the coefficients of which should contain unknown quantities. And from this time I had no further difficulty in arriving at the remarkable theorem announced at the end of the third part of the Researches, *that any number of general algebraic functions which are of* n', n'', n'''... *dimensions relatively to an assignable number of unknown quantities contained in them, can be made simultaneously equal to zero, by means of equations of* ..., n',..., n'', ..., n'''... *dimensions**.

I was thus enabled to perceive that the general equation of the mth degree might be reduced to the form $y^m + \mathrm{K}' y^{m-n} \ldots + \mathrm{V}' = 0$, without the aid of an equation of more than $(n-1)$ dimensions. I found too that when m, A, B, C, ... V were indeterminate†, neither would the expression $\frac{v}{0}$ occur

* A *demonstration* of this theorem, very nearly according with that which had suggested itself to my own mind, has lately been sent in a letter to my brother Dr. Jerrard, by V. F. Hovenden, Esq., late Fellow of Trinity College, Cambridge.

† In the Supplement to Part III. of the Mathematical Researches, I speak of reducing the general equation of the *fifth* degree to De Moivre's form. But the problem which I perceived to be solved when the non-

among the quantities P, Q, R, ..., nor the series for y become a multiple of the primitive equation $x^m + A\,x^{m-1} + B\,x^{m-2} \ldots + V = 0$.

[To be continued.]

LXV. *Proceedings of Learned Societies.*

OFFICIAL REPORT OF THE PROCEEDINGS OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, AT THE DUBLIN MEETING, AUGUST 1835.

Communicated by the Council and Secretaries.

[Continued from p. 411.]

Notices and Abstracts of Miscellaneous Communications to the Sections, continued.

GEOLOGY AND GEOGRAPHY.

On the Geological Map of Ireland. *By* R. J. Griffith.

Mr. Griffith presented his Geological Map of Ireland, the result of many years' research and labour, assisted in part by the publications of Weaver, Conybeare, Buckland, and Berger. Mr. Griffith, in pointing out the inaccuracies of existing maps of Ireland, dwelt on the advantages which will be derived from the publication of the Ordnance maps of Ireland, four counties of which have now appeared. At present great difficulties attend the allocation of geological phænomena, which are frequently misplaced in relation to each other, from the necessity of following the defects of the old maps. Mr. Griffith, as an example, stated that in Arrowsmith's map, Benwee Head is placed twenty miles north of the parallel of Sligo, though it is actually due west of that town. The remarkable position of the mountain masses was first pointed out. They occur on the margin of the island, and inclose the great central limestone plain; an arrangement which shortens the courses of the rivers, rising as they do in the higher grounds, and rapidly descending to the sea. The Shannon is an exception, having a course of 140 miles; but it also is affected by the peculiarity alluded to, its stream falling eighty feet in the first twenty miles of its course, and only eighty feet more in the remaining 120. On the great plain which occupies the centre of the island numerous beds of gravel occur, called Escars, which though constant in direction when considered in reference to small spaces, are variable when the comparison extends over greater limits. Mr. Griffith considers the great bogs as due to these accumulations of gravel, which, by damming in the water, facilitate the growth of *Sphagnum palustre*. Under the bogs are deep deposits of marl, underlaid by clay and gravel, which further support the idea of ancient lakes. The marl was stated to be in one instance forty feet

occurrence of the expression $\frac{v}{0}$ was taken into consideration was this, *to reduce the general equation of the* m*th degree to a form which would coincide with De Moivre's, if* $m = 5$.

thick. Mr. Griffith, confining himself on this occasion to the sedimentary rocks, commenced his illustrations by those of a more crystalline character, such as gneiss, mica slate, &c.; and stated that he considered the great groups of Ireland as corresponding to those of Scotland, particularly the Northern to the Grampians, and the Mourne to the Dumfriesshire mountains. The general direction of stratification is N.E. and S.W., though in Tyrone it is more nearly N. and S., being referred to a local axis; and in the south nearly E. and W. The beds of primary limestone, associated with the primary schists, are not continuous, though they occur in lines: when intersected by trap dykes, they become dolomitic. The quartz rock, which is also associated with these schists, is sometimes very remarkable. At Dunmore Head it has the structure of orbicular granite, or of some varieties of trap, for which it is often mistaken. Mica slate is unequally distributed: it is abundant in the north and west, less general in the south, and deficient in the Mourne or Down district. Mica also, as a mineral, is not general, being in the Mourne mountains often replaced by hornblende. Proceeding to the transition schists, Mr. Griffith stated his conviction that they would require subdivision, whenever materials had been collected for the purpose, in the same manner as those of Wales had been divided by Mr. Murchison. For example, in the older schists, neither conglomerates nor organic remains are found. In the newer greywackes, the slates alternate with sandstone; and again, in the still newer strata, limestone, containing fossils, alternates with the upper portion of the schists. The old red sandstone is also considered by Mr. Griffith divisible into two or three subsections, the upper alternating with the mountain limestone. Mr. Griffith then described the several coal-fields of Ireland, pointing out the distinction between those of the north and south, bituminous coal being confined to the northern collieries. The more recent sedimentary rocks were then briefly described, more especially the new red sandstone, which underlies the lias and chalk on the S. and E. of Antrim, and is also found in Monaghan, and may be traced thence through Tyrone and Derry to Lough Foyle, and round Lough Foyle to Donegal.

Having previously described the sedimentary, he now entered on a description of the crystalline rocks considered as rocks of intrusion. In the Wicklow range, extending to Brandon, the granite contains no hornblende, and, as previously noticed by Mr. Weaver, occurs sometimes as beds in mica slate. In the Mourne or Down range, the granite contains hornblende, which frequently predominates over the mica. In Wicklow, mica slate, passing into gneiss and clay slate, abuts without disturbance against the granite. In Down mica slate is wanting, and the other schistose rocks are frequently disturbed. In western Donegal mica slate and quartz rock are abundant, the quartz rock being developed to a great extent; and in Galway also, associated with mica slate, quartz rock is extensively diffused. In both these counties granite occurs, and the crystalline stratified rocks are referred to as affording distinctive characteristics of its several localities. The phænomena usually exhibited by granite

veins are frequently observable, such as their passage through the adjacent schists, detached portions of which are often enveloped in their substance, and the change they effect in their structure. Mr. Griffith next described the older and newer trap districts, mentioning many interesting particulars connected with them, such as the capping of quartz rock by greenstone, the concentric arrangement of the beds of greenstone in Donegal, and the occurrence of quartz rock between two beds of greenstone, the quartz being columnar, the trap, above and below it, not. In Slieve Gullin greenstone and granite were stated to be actually mixed together, whilst in Carlingford the contact of the sienite (or greenstone) with the granite is concealed by debris. After noticing briefly the ochre beds which so often separate the beds of basalt, and expressing his belief that the trachytic porphyry of Sandy Brae in Antrim was nothing more than this ochre indurated by heat, Mr. Griffith adduced the fact of beds of sienite traversing the cliffs of Murloch Bay, and containing detached portions of chalk, as proof that the sienite was posterior in appearance to the chalk; and gave it as his opinion that all the crystalline rocks had been fused, and in most cases projected from beneath through the sedimentary rocks, the appearance of regular strata being due to their projection in a direction parallel to the strike of the beds.

Mr. Griffith stated the existence of an extensive marl deposit in Wexford, some of the shells of which appeared to correspond with those of the crag.

On a small isolated Patch of Granite which occurs in the County of Cavan. By Lieutenant STOTHERD.

The superficial extent of this granite is about seven square miles, and it is separated from the nearest group of primitive rocks, that of the Mourne mountains, by the grauwacke or transition schists. This small district is entirely surrounded by transition and secondary rocks, and exhibits all those changes in the structure of the sedimentary rocks which are usually observed on their approach to, or contact with, rocks of a decidedly igneous origin, the schists becoming indurated and often changed to quartz rock. The appearance of primary rocks so far removed from any of the greater masses is extremely important in geological speculation, and assists in this instance in explaining the broken and detached character of the schistose hills, and the induration of many of their strata, since it is probable that the granite is at no great distance from the surface in the whole space between the Cavan primary rocks and the Mourne mountains, of which they may be considered an extension.

Copies of a map of the geology of the environs of Dublin, accompanied by a memoir, were presented to the Section by Dr. SCOULER, Professor of Geology to the Royal Dublin Society.

On Eleven Trap Dykes in the Counties of Mayo and Sligo, running East and West for great distances. By Archdeacon VERSCHOYLE. (Printed in Proceedings of Geological Society.)

On certain Fossil Polyparia *found in Alluvial Deposits in the vicinity of Limestone Hills. By Dr.* JACOB.

The specimens were *Lithodendra*, of the species usual in the carboniferous limestone of England, the coralline lamellæ being replaced by silica, and the limestone partially removed by water containing carbonic acid. Similar cases are frequent in the North of England: the circumstances under which they occur appear to Dr. Jacob to deserve special inquiry.

On the Silurian *and* Cambrian Systems, *exhibiting the order in which the older Sedimentary Strata succeed each other in England and Wales. By Professor* SEDGWICK *and* R. I. MURCHISON, *V.P.G.S.*

Mr. Murchison described a great group of fossiliferous deposits which rises out from beneath the old red sandstone. To these rocks, which he has termed in descending order the *Ludlow*, *Wenlock*, *Caradoc*, and *Llandeilo* formations, (each distinguished by peculiar organic remains, and frequently by subordinate limestones,) it was found essential to assign a comprehensive term, since they constitute one natural system interpolated between the old red sandstone and the slaty rocks of Wales. He observed that it was well known to all practical geologists, that in consequence of the recent advances of the science, it was absolutely imperative that the term "transition", under which such rocks would formerly have been described, should now be abandoned, since it had been so used, both by Continental and English writers, as to embrace the whole carboniferous series, from which the system under review was not only separated by the vast formation of the old red sandstone, but was specially to be *distinguished* by its fossil contents. Urged, therefore, by many geologists to propound an entirely new name for the class of rocks which had engaged his attention during the last five years, Mr. Murchison recently suggested (See Lond. and Edinb. Phil. Mag., July 1835, pres. vol. p. 48.) that the group should be termed the "*Silurian System*," the name being derived from the ancient British people, the Silures, who under Caractacus made so noble a stand against the Romans, and within whose territory the rocks under consideration are fully displayed. Mr. Murchison then pointed out, that wherever the limestones and typical characters of particular formations were absent or obscure, it was always practicable, over a region of 120 miles in length, extending from the neighbourhood of the Wrekin and Caradoc hills, in Shropshire, to the west coast of Pembrokeshire, to separate the groups into two parts, the "Ludlow" and "Wenlock" formations, forming the "*Upper Silurian*," the "Caradoc" and "Llandeilo" the "*Lower Silurian rocks*". He further remarked, that in South Wales he had traced many distinct

passages from the lowest member of the "Silurian system" into the underlying slaty rocks, now named by Professor Sedgwick the "*Upper Cambrian.*"

This communication was illustrated by Ordnance Maps extending over large parts of eleven counties, coloured in the field by Mr. Murchison.

Professor Sedgwick commenced by pointing out the imperfection of the sections exhibited in the North of England, and some portions of North Wales, in consequence of the entire want of continuity between the carboniferous series and the inferior schistose groups. Some of the latter are fossiliferous both in Denbighshire and Westmorland; but in the interrupted sections of those counties it is impossible to tell how many terms are wanting to complete the series to the old red sandstone and carboniferous limestone. In the country described by Mr. Murchison these difficulties do not exist, and his sections have filled up a wide chasm in the succession of British deposits. Professor Sedgwick then described in descending order the groups of slate rocks, as they are seen in Wales and Cumberland. To the highest he gave the name of *Upper Cambrian group.* It occupies the greatest part of the chain of the Berwyns, where it is connected with the Llandeilo flags of the Silurian system, and is thence expanded through a considerable portion of South Wales. In one part of its course it is based on beds of limestone and calcareous slate; but on the whole, it contains much less calcareous matter than the Silurian system, and has fewer organic remains. Beds of good roofing-slate occur, and a perfect slaty cleavage is often observed in it transverse to the stratification; but other parts of it are of a coarse mechanical texture. To the next inferior group he gave the name of *Middle Cambrian.* It composes all the higher mountains of Caernarvonshire and Merionethshire, and abounds in fine roofing-slate, alternating with, and apparently passing into, irregularly interstratified masses of porphyry. Some portions of it are coarse and mechanical, and it contains (for example, at the top of Snowdon,) a few organic remains, and a few examples of highly calcareous slates, but no continuous beds of limestone. The same group, with the same mineral structure, and in the same position, but without organic remains, is greatly developed in Cumberland. The *Lower Cambrian* group occupies the S. W. coast of Caernarvonshire, and a considerable portion of Anglesea: it consists chiefly of chlorite schist, passing here and there into mica schist and slaty quartz rock, and contains subordinate masses of serpentine and white granular limestone. It contains no organic remains. Beneath the *Middle Cambrian* system (above described) there occurs in Cumberland (for example, Skiddaw Forest,) a great formation of dark glossy clayslate, without calcareous matter, and without organic remains. It passes in descending order into chiastolite slate, mica slate, hornblende slate, gneiss, &c., which rest immediately on granite. Whether the *Lower Cambrian* was to be placed on the exact parallel of these masses in Skiddaw Forest, the Professor did not determine.

Professor Sedgwick explained the mode of connecting Mr. Murchison's researches with his own, so as to form one general system. He pointed out also the limit, as at present known, of fossils, none having been hitherto discovered in the *Lower Cambrian* schists, and remarked in reviewing the general phænomena, that geological epochs were not effected by shocks, but, like everything in nature, were under the dominion of the usual laws of causation.

Notices of the Geology of Spain. *By Dr.* Traill.

The author gave a sketch of the results of his personal researches in the geology of Spain, restricting himself, however, to a few only of the more striking peculiarities. He stated that it was an error to suppose all the mountain chains of Spain branches of the Pyrenees, from which they are in many cases completely separated. The variety of climate, and circumstances produced by the union of these mountains with the elevated table lands of New Castile, which is two thousand feet, and of Arragon, which is two thousand five hundred feet above the sea, had very peculiar effects on the flora of the country. Dr. Traill pointed out the identity of character which existed between the granites and schists of Spain and England, and proceeded to the newer strata; described the brine springs and salt lakes of Andalusia, and the deposit of salt which forms part of the base of the plain of Grenada. He also showed that lias and true chalk, with layers of flint, occur in the South of Spain, and confirmed the statements by Colonel Silvertop, of the tertiary deposits of Spain. Dr. Traill further observed, that bones are found in the fissures of other hills in Spain besides that of Gibraltar.

On certain Disturbances in the Coal Strata of Yorkshire having a remarkable Relation to existing Valleys; illustrated by a Map and Sections. *By* Henry Hartop.

M. Agassiz presented the fourth and fifth livraisons of his work on Fossil Fishes, and stated, that by the great addition of 300 species which had been obtained from the cabinets of these countries, the total number had been raised to about 900. He then advanced some general views on the conclusions to be drawn from the geological distribution of fishes, and explained the precision in determining epochs which their higher state of organization and consequent susceptibility to external influences afforded. The fishes of the carboniferous period were different from those of the lias; the fishes of the lias different from those of the oolite; and those of the oolite from the fishes of the chalk: and as it must be presumed that fishes living together so coexist from the necessity of their organization, and its adaptation to attendant circumstances, it must also be presumed that their disappearance was the result of a change in the conditions of the earth's surface. In estimating the effects of such changes, it is necessary, M. Agassiz observed, to distinguish between general phænomena affecting, as it were, the laws of nature, and

those of a mere local character, such as volcanic eruptions. The local phænomena may indeed have been similar to those of the present time, but the elevations of mountain chains are evidences of a more general class of phænomena, which have affected organic life, constituting thereby the various zoological epochs which may be traced in the earth's strata. It was in such periods of violence and change that the beds of any one system were deposited, the animals coexisting at the time being, according to the more or less susceptible nature of their organization, more or less completely annihilated; and it was in the tranquillity which followed, that new beings were formed, and lived to tenant in like manner the strata of another system, which should result from another epoch of disturbance. M. Agassiz produced, as an example of sudden destruction, a drawing of fossil fishes crowded together in a very confused manner, such as could only have arisen from an instantaneous catastrophe, arresting them, as it were, in a moment.

M. Agassiz then, at the request of Professor Sedgwick, explained those characters, such as the position of the fins, the arrangement and size of the scales, &c., by which the fishes of different geological eras may be distinguished, referring especially to those of the old and new red sandstones.

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1. *On British Fossil* Astacidæ, *their Zoological and Geological Relations.* 2. *On British Belemnites. By* JOHN PHILLIPS, *F.R.S., G.S., Professor of Geology in King's College, London.*

(The leading results of these two communications, which form part of a general investigation of British organic fossils, undertaken at the request of the Association, will be given in the next volume of Transactions.)

Notice of a newly discovered Tertiary Deposit on the Coast of Yorkshire. By JOHN PHILLIPS, *F.R.S., &c.*

Two hundred yards north of the harbour of Bridlington, near the situation where Professor Sedgwick and the author and other observers had *suspected* and *looked for* tertiary beds, a wasting of the low cliff had disclosed to a small extent layers of greensand and clay, both, but especially the former, containing shells, &c. Diluvial clay and pebbles cover and partially confuse this deposit. Of 55 species of fossils from these beds, which are in Mr. Bean's cabinet at Scarborough, a very small number (four) belongs to the crag, a very small number (five or six) to recent species, and the greater proportion is extinct. On comparison of the facts known concerning this deposit, the crag, the Touraine beds, and certain other foreign tertiaries, Professor Phillips founded an argument concerning the limits of error in the application of Mr. Lyell's test of the age of tertiary formations by the numerical relations of the species of fossils which they contain to recent forms. It appeared to Professor Phillips that these limits were wide, and that a method of such power and value must not be applied without great caution.

A letter from Charles Lyell, F.R.S., Pres. G.S. to Professor Sedgwick, on the fossil shells of the Suffolk Crag, considered in two divisions, according to the views of Mr. Charlesworth*, was read to the meeting.

Account of Fossil Trees in the attitude of growth in the Coal Measures near Glasgow. By James Smith, *F.R.S., of Jordan Hill.*

The trees in question were discovered at Balgray Quarry, immediately adjoining the aqueduct over the Kelvin, about three miles to the north of the city of Glasgow.

The quarry abounds in the usual coal plants, laid horizontally; in one part of it a number of trees were found standing in an upright position, throwing their roots out in all directions, to all appearance in the attitude in which they grew, without fracture or disturbance. They rest upon, and are imbedded in, strata of sandstone, which are horizontal, or nearly so. The stems terminate about two feet above the roots, the superincumbent bed of stone passing over them as if they had been cut off. They are about two feet and a half in diameter, and are placed as near each other as trees of the same size could grow. No internal structure was observed, but from the ramification of the roots and of fragments of branches found near them, and the external appearance of the bark, which is channeled or furrowed, the author presumes that they were dicotyledonous.

On certain Fossil Plants from the opposite Shores of the Bristol Channel. By the Rev. David Williams, *F.G.S.*

These fossils were collected by the author in Devonshire and Pembrokeshire, from shales alternating with anthracitic coal (*culm*); and he states that, after a careful examination, he was led to conclude positively that the strata of the true localities belong to very different geological æras, that of Devon extending from Bideford to South Molton, being a true 'transition' coal, imbedded in 'transition' schists, and that of Pembrokeshire and Caermarthenshire belonging to the coal-measures above the mountain limestone. On the similarity of these plants found in formations of such different age, the author founds objections to the hypothesis of secular refrigeration; and the speculation that the atmosphere in early geological periods was charged with a greater quantity of carbonic acid gas; and proposes the case as at least an exception to the law, that strata may be identified by their imbedded organic remains.

On the Survey of the Mersey and the Dee. By Captain Henry Mangles Denham, *R.N., Resident Marine Surveyor of the Port of Liverpool.*

Captain Denham exhibited his trigonometrical survey of the Mersey and Dee, including the extensive sand-banks and channels of Liverpool bay, which, being delineated on the scale of four inches

* See our present volume, p. 81 *et seq.*, p. 353—354, and also p. 461, *et seq.* of our present number.—Edit.

to the mile, afforded a detailed development of the submarine undulation, illustrative of his remarks on the action of the tidal stream in connexion with those differently shaped estuaries. The self-choking effects of the Dee, with its expansive mouth and gradual contraction, resembling a lateral section of a cone, were contrasted with the scouring effects of the Mersey, its contracted mouth and attenuated throat resembling a lateral section of a bottle with its neck pointed seaward. To this figure of the estuary of the Mersey, Capt. Denham ascribed the impetus of its expansive back-water, which has recently forced a channel of half a mile wide, and two miles long, and twelve and thirty feet below the low-water level, through sands, situated eight miles outside its coast-line confines, *at a tangent to its regular course.* Thus a most valuable and unexpected channel has been produced for navigation, and a compensating escape provided for its waters at a time when an injurious deposit was taking place across its usual path, where the efforts of the ebb become evanescent. The position was ascertained by Captain Denham to be fourteen miles below the docks, or tidal straits, where the first impulse amounts, (and continues so five hours out of six) to five miles per hour on spring-tides. The form of this channel corresponds to the contour of incidence and reflection throughout its whole course, and indicates the exhaustion of the velocity of the water by expansion in the proportion of 14 to 25. It proves also the certain power of the Mersey to command a *navigable* avenue to the ocean, so long as its guardians preserve the high-water boundaries from artificial contraction.

In the course of his professional duties, Capt. Denham proposes to himself a further investigation of the proportions of silt, &c. held in suspension and gradually deposited, as well as a determination of certain peculiarities in the *vertical* range of the tides with reference to atmospheric elasticity. He has already, by the liberal arrangements of the dock-trustees, been enabled to connect a series of observations, even to *five-minute grades*, during the twenty-four hours. From these, by extensive tabulary interpolations, the half-hourly rise and fall upon every stage of the moon was determined, and the mariner enabled at a glance to know what water existed in excess of his chart, and hence *when* certain subsidiary channels were passable, or the several banks might be crossed. He had thus ascertained the tidal *establishment*, or the time of high-water upon full and change of the moon, and determined another constant proportion as a standard—for graduating future tide-gauge operations, for testing soundings hereafter, for fixing a point of departure for engineers when levelling eminences, canals, railroads, &c.,—viz. the oscillating point, or mean centre which every six hours is common to neaps and springs, and quoted by seamen generally as the *half-tide mark.* Capt. Denham is not as yet prepared to state whether some *small* constant difference might not be found as to the *instant* of the half-elapsed time of spring-tide, high and low water, and that of neaps, producing the actual *half-range* of tide to *inches*; but so satisfied is he of a closer approximation than is generally allowed, that, though he would never propose *to adjust soundings to that*

half-tide level, because the mariner would have to make variable allowances to ascertain the least water he was to expect in the channel before him, yet he would suggest for scientific and frequent practical references the desirability of engraving on some rocky spot of every harbour, and sheltered portions of coast, the well-defined *half-tide level*, DATED; for, on the assumption that such a level is (no matter what the whole amount of rise and fall differs), in the same latitude, equidistant from the earth's centre, then we have a standard of obvious importance to science. By reference to this constant level those discrepancies may be adjusted which attend engineering operations, designed to cooperate on opposite sides of an isthmus, where the vertical range differs, and either *high* or *low* water level *separately* be started from, instead of the mean centre of *each range*, i.e. *half-tide* level.

The Rev. WM. WHEWELL made the following remarks for the purpose of exemplifying the application of physical science to geological researches.

1. The permanence of the level of mean water, which Capt. Denham has recently proved by trial at Liverpool, suggests the proper mode of making such observations on the permanence of the relative level of land and sea, as were formerly recommended by the Association. In tidal seas the level of the ocean must, for such a purpose, be estimated with reference, not to the height of high or of low water, which is variable on many accounts, but to the height of *mean water*. This mean water is to be obtained by taking at least two high waters and the intervening low water, or two low waters and the intervening high water. A very few tides will give a near approximation to the true mean level; but the more there are taken, the more accuracy will be obtained. This mean level must, of course, for the purposes now spoken of, be referred to some durable mark in the solid ground. 2. The phænomena of terrestrial magnetism, being apparently connected with the internal constitution of the earth, are of interest to the geologist. According to the most recent researches of Hansteen the earth has four magnetic poles, all of them revolving in the neighbourhood of the geographical poles; and the periods of these revolutions are respectively about 4600, 1740, 1300, and 860 years. These times, though long as historical periods, are short compared with many of those cycles of which geological researches and astronomical calculations prove the existence; and it is impossible not to feel a great curiosity respecting the nature of the subterraneous changes which take place in such periods. It concerns the geologist therefore, no less than the physical philosopher, to further the progress of our knowledge of terrestrial magnetism. 3. The heat of the interior parts of the earth has always been treated of by those who have established the theory of heat upon mathematical principles. They have hitherto considered it as proved, upon such principles, that the increase of temperature of the substance of the earth as we descend, proves the reality of an *original heat*. But M. Poisson, in his

Theorie de la Chaleur just published, dissents from this opinion, and is disposed to assign another reason for the higher temperature below the surface. He observes that the cosmical regions in which the solar system moves have a proper temperature of their own; that this temperature may be different in different parts of the universe; and that if this be so, the earth would be some time in acquiring the temperature of the part of space in which it has arrived. This temperature will be propagated gradually from the surface to the interior parts. And hence, if the solar system moves out of a hotter into a colder region of space, the part of the earth below the surface will exhibit traces of that higher temperature which it had before acquired. And this would by no means imply that the increase of temperature goes on all the way to the centre. Though these opinions may not gain the assent of geologists, it may be proper that they should be aware that such have been promulgated.

On the Geographical Position of Cape Farewell. By Dr. West.

The chief object of the memoir was to show, That Cape Farewell, so named by Davies in 1585, is not, as stated by Egede, Crantz, and Giesecke, on the island of Sermesok, but on another island many miles to the south-east of it;—That Staten Hoek is not, as generally laid down in charts, a promontory on the southernmost extremity of the main land, nor yet, as stated in the Edinburgh Review (No. 59,) an *inlet*, but that it is identical with Cape Farewell, and received its name, which signifies the *States' Promontory*, from the Dutch navigators. Dr. West also showed that this fact, though now apparently quite unknown in these countries, was understood and plainly stated nearly ninety years ago in an English work, Drage's Account of the Voyage in the California in 1746 and 1747.

The memoir was accompanied by a copy of Graah's Chart of Greenland, the latest and most correct extant, from which it appeared that Giesecke, in his account of Greenland in Brewster's Edinburgh Cyclopædia, and in his map of that country in the 14th vol. of the Transactions of the Royal Irish Academy, has placed the island of Sermesok nearly a degree too much to the south; that no part of the main land could possibly be seen from the open sea to the south of the coast of Greenland; and that the island most to the south of the strait Ikareseksoak is the only one on which is a cape answering to the description given by navigators of Cape Farewell.

Dr. West concluded his memoir by expressing his opinion that Captain Graah, by his having satisfactorily ascertained that there was no trace whatever of a colony on the east coast from its southernmost extremity to lat. 65° 30′, has completely established the correctness of the opinion of Eggers that the Œsterbygd, or eastern settlement, was situated on the south-west coast, in what is now Julianeshaab's District; and that it received its name merely from the fact of its being to the east of the other settlement, the Vesterbygd.

ZOOLOGY AND BOTANY.

On the Principles of Classification in the Animal Kingdom in general, and among the Mammalia in particular. By Professor Agassiz.

Although the principal groups of animals are impressed with such characters as to be easily recognised and to admit of little doubt, yet their order and succession have been determined *by no general principle.* This appears from the discrepancy in the position assigned to them by the most eminent systematists, each of whom has assumed *arbitrarily* some organ or system of organs for the basis of his arrangement. Professor Agassiz, after adverting to some German naturalists who alone have sought after a general principle which should be satisfactory to "philosophic naturalists," passed in review the classes of the animal kingdom, each of which, he stated, exhibited in an eminent degree the development of some one of the animal functions. While Vertebrate animals (with Man their type) arrive at the greatest perfection in the organs of the Senses, the Invertebrate offer in the class of Worms the representative of the system of Nutrition, in *Crustacea* of Circulation, in Insects of Respiration, and in *Mollusca* of Generation. The Professor next proceeded to demonstrate in what manner each subclass of vertebrate animals derives its peculiar character from some one element of the animal œconomy.

This predominant element is the bony skeleton in Fishes, the muscular structure in Reptiles, the sensibility of the nervous system in Birds, and the perfection of the senses in *Mammalia*, which therefore reproduced the distinguishing character and constitute the type of vertebrate animals. He next showed that each of the other subclasses of the higher group is represented among the *Mammalia* along with its own peculiar type. He explained his reason for the fourfold division which he had adopted in the subclass, pointing out the close affinity which connects the *Ruminantia*, the *Pachydermata*, the *Rodentia*, the *Edentata*, and the herbivorous *Marsupialia*, (in none of which is the true canine tooth developed,) which he considers as forming a single group; in another he unites those characterized by the presence of the canine tooth in its proper function (as an instrument of nutrition, not merely of defence), viz. the *Carnivora* and those *Marsupialia* which partake of their character, and the *Quadrumana*. The *Cetacea* form a group in themselves; and Man another. The manner in which these represent the subclasses of *Vertebrata* was exhibited by the comparison of

Cetacea,	with	Fishes,
Ruminantia, &c.		Reptiles,
Carnivora, &c.		Birds;

while Man is the perfection and type of the mammiferous conformation.

Prof. Agassiz then applied this principle to illustrate the order and succession of the groups in *Mammalia* by a reference to the or-

der in which the fossilized remains of the *Vertebrata* occur in the stratified deposits: 1. Fishes, 2. Reptiles, 3. Birds, 4. Mammalia. From the same consideration results the following arrangement of the representative groups among these last: 1. *Cetacea*, 2. *Ruminantia*, &c., 3. *Carnivora*, 4. Man, who thus in a twofold aspect becomes the culminant point of the animal creation.

Observations on the Zoology of the Island of Rathlin, off the Northern Coast of Ireland. By JAMES DRUMMOND MARSHALL, *M.D.*

The zoology of Rathlin does not offer any new species in addition to those hitherto found on the opposite coast of the county Antrim, and this notice was laid before the Association rather to mark the *habitats* of some species than to add anything to what is already known.

The only *Mammalia* frequenting the island are, the Norway Rat, the Common Mouse, the Shrew Mouse, and the Hare. The latter is but rarely seen, and not being able to procure a specimen, the author cannot say whether it is the hare of Great Britain or that lately ascertained to be a species, or rather perhaps a *variety*, peculiar to Ireland.

In *Ornithology*, so far as the author could ascertain, there are about 60 species, comprising 32 land and 28 water birds. From the situation of the island, its precipitous cliffs, and the consequent facilities for incubation, many species of water birds choose it for a summer residence. The most common species are the *Larus Rissa*, *Larus argentatus*, *Larus Canus*, *Alca Torda*, *Fratercula arctica*, *Uria Troile*, *Uria Grylle*, *Phalacrocorax Carbo*, *Phalacrocorax cristatus*.

Although all the above-mentioned species are plentifully distributed, the *Larus Rissa*, or Kittiwake, is by far the most numerous; every headland round the northern shore of the island was tenanted by this common though beautiful species. In company with it were found the *Alca Torda*, *Fratercula arctica*, and *Uria Troile*, all living in harmony with each other; the Puffins occupied the earthy patches which here and there occurred between the basalt and limestone of which the rocks are chiefly composed, while the three former tenanted every pinnacle and ledge of rock not otherwise occupied. The *Uria Grylle* inhabited one of the headlands on the southern extremity of the island; but their numbers were by no means equal to those of the *Uria Troile* or *Arca Torda*. The myriads of fry of different species of fish, particularly the *Launce*, or Sand-eel, furnish an ample supply of food to the various sea-fowl frequenting Rathlin.

The *Fishes* of this island do not differ from those found on the northern shores of Ireland. One of the most common species is the Coal-fish (*Gadus carbonarius*). This on the Irish coast is called, in its different stages of growth, *Pickoc*, *Blochan*, *Glashan*, and *Grey Lord*, and corresponds, according to Dr. Neill, to the *Sillock* and

Piltock of Shetland, the former name being applied to the fry, and the latter to the fish when a year old.

The Cod-fish is but rarely procured, there being but one cod-bank (which is called *Skirnaw*), lying between Rathlin and Isla in Scotland.

The Lithe, Ling, Plaice, and Turbot are occasionally caught; and during summer the Grey Gurnard and one or two species of Wrasse are plentiful round the shores.

The Fifteen-spined Stickleback (*Gasterosteus spinachia*) has been found in the pools on the shore, and in the rivulets and ponds the Short-spined Stickleback (*G. brachycentrus*).

Notices of the Geographical Range of certain Birds common to various Parts of the World but principally to India and Europe. By Lieut.-Col. W. H. SYKES, *F.R.S.*

Circaëtus brachydactylus, Vieillot.	India and France.
Aquila chrysaëta	India and Europe.
Falco Tinnunculus	India and Europe.
—— *Chicquera*	India and Cape of Good Hope.
Circus cyaneus	Europe, and only slightly differing in India.
Strix Javanica	India, Java, and Cape of Good Hope. (Very like *Strix flammea* of Europe.)
A Swallow hardly distinguishable from *H. rustica* of Europe ...	India.
Alcyon Smyrnensis	Smyrna and India.
Alcedo rudis	Dukhun and Cape of Good Hope.
Muscipeta (longtailed white and chestnut)	South Africa and India.
Collurio Excubitor	Europe and North America. A species or variety in India very slightly different.
Oriolus Galbula	Europe, India, and Cape of Good Hope.
——— *melanocephalus*	India and the Cape.
Cape Thrush (*Ixos Caffer*)	India and the Cape.
Ixos falcatus	Dukhun and Philippines.
Lesser Whitethroat	
Budytes citreola	
Stonechat	
Phœnicura Suecica	
Emberiza melanocephala	All found in Europe and India.
——— *hortulana*	
Common Sparrow	
Pastor roseus	
Coracias Indica	India and the Cape.
Hoopoe (not of Europe)	Cape and India.

Leptosomus afer	Cape and Dukhun.
Cuculus fugax *Centropus Philippensis*	Eastern Islands and Dukhun.
Cuculus canorus	Europe and Dukhun.
Cinnyris aurucaria	Cape and Dukhun.
——— *Mahrattensis*	Philippines and Dukhun.
Columba risoria	Senegal, India.
——— *œnas*	India, China, Europe.
Peafowl	Wild in India.
Common Fowl	Ditto.
Coturnix dactylisonans	China, India, Cape, Arabia, Barbary, Europe. (Not migratory in India and the Cape.)
Pterocles exustus	Asia Minor, India.
Francolinus spadiceus	Madagascar and India.
Several species of Herons	Common to India, the Cape, and Europe, or to two of these countries.

The Sacred Ibis of Egypt is believed by Col. Sykes to be the same as the Indian Ibis.

Ibis falcinellus	Europe and India.
Green Sandpiper Wood Sandpiper Common Sandpiper	Common to Europe and India.
Totanus Ochropus	Hudson's Bay and India.
Common Snipe Jack Snipe	India and Europe.
Rhynchæa	Cape and India.
Pelidna Temminckii	India and Europe.
Jacana	China and India.
Gallinula	Java and India.
Porphyris	Madagascar and India.
Coot	Europe and India.
Cursorius Asiaticus	India and Cape.
Golden Plover	N. America, Europe, India.
Himantopus melanopterus	Java, India, Europe.
Anas strepera *Rhynchaspis virescens* *Mareca fistularis* *Querquedula circia* ——— *Crecca* *Fuligula rufina* ——— *cristata*	India and Europe.
Sterna Anglica	North coasts of Great Britain and Dukhun, 100 to 200 miles inland, and 1800 feet above the sea, with similar changes of plumage from summer to winter.

Besides the instances of *identity* above quoted from specimens in

Colonel Sykes's own cabinet, others are mentioned of such *close analogy* as to render their specific difference extremely dubious. Many species of birds of different natural groups and habits are thus proved to have an extensive geographical range, under considerable differences of mean temperature. Deducting those species, which do or may be imagined to migrate from one region to another, there remains abundant evidence, derived from continually resident birds, that some birds live in India with a mean temp. of 77° to 82°, and in Britain with a mean temp. of 45° to 50°. Connecting these facts with the instances of tigers living near the limits of perpetual snow, and elephants and Indian birds braving our winters, Colonel Sykes concludes that the power of acclimation possessed by many birds and other animals is very considerable, and capable of useful application to a question of practical importance, viz. the necessity of employing artificial heat generally in our vivariums, and to the curious geological problem of the climate of the globe when elephants and tigers were inhabitants of the northern zones.

[Captain James Ross, in corroboration of these views, stated that the Stonechat, Whitethroat, and Golden Plover were inhabitants of Hudson's Bay, and that the Raven also occurs in the Arctic Circle, without being subject to change of plumage.]

On the Infra-Orbital Cavities in Deers and Antelopes. By Dr. Jacob.

[This paper having been drawn up in compliance with a recommendation of the Association, will be printed in the next volume of Transactions.]

On a Mode of preserving Echinodermata. *By the Rev.* Charles Mayne.

In the year 1828, being at the sea-side, Mr. Mayne collected many *Echini* for examination; and the house not being large enough to afford him a separate room, he used chloride of lime to prevent inconvenience to the family from the smell. He soon perceived that the *Echini* steeped in the solution did not lose their spines; he accordingly tried to preserve them with all their spines on, and succeeded completely. He has since tried this process with many *Echini* and small star-fish. The preparation should not be so strong as to act sensibly on the surface of the crust, as in that case he found that the spines would fall off.

On Pentacrinus Europæus *and a Species of* Beroë *taken in Dublin Bay. By* R. Ball.

Specimens of these were exhibited to the Meeting. The *Beroë* has been examined by Mr. R. Patterson of Belfast, who finds it to be a new species of the genus *Pleurobrachia* of Fleming. It has been also taken in Larne Lough, Antrim.

The Rev. Dr. Drummond stated that, from observations lately made by him, the *Gordius aquaticus* seems to be viviparous.

On the Action of Light on Plants. By Professor Daubeny.

Professor Daubeny reported the progress which he has made in his experiments on this subject since 1833, when he communicated the results obtained up to that time to the British Association at Cambridge. At that period he had ascertained that the quantity of carbonic acid decomposed by a plant was in proportion, not to the chemical or heating influence of the ray transmitted to it, but to its illuminating power: he has since found that the functions of exhaling moisture by the leaves, and absorbing it by the roots, depend upon the same law; with this difference, however, that, provided some light be present, a body radiating much heat will serve as a substitute for one transmitting a greater degree of light. Thus, a solution of ammonio-sulphate of copper, which absorbs and consequently radiates much heat, is nearly as efficient in causing the exhalation and absorption of moisture as glass, which transmits the entire spectrum; and in proof that this does not depend upon any peculiar power residing in the violet ray, water obscured by ink, so as to produce an equally feeble illuminating effect, was found, in consequence of the heat it radiated, to produce an equal degree of exhalation. Yet when the plant was covered over by opake bodies radiating much heat, the amount of moisture exhaled was very inconsiderable.

Professor Daubeny has employed, in his experiments on plants, the light emitted by balls of lime ignited by the oxy-hydrogen jet, but could not discover that it exerted any influence on the quantity of moisture exhaled by them.

Observations on the Structure of Horizontal Branches of Coniferæ. *By* William Nicol.

In a paper on the structure of recent and fossil *Coniferæ*, inserted in Professor Jameson's Philosophical Journal for January 1834, the author gave an account of a very striking difference he had observed in the structure of the opposite sides of a piece of the wood of *Taxodium disticha*. The pith was much nearer one side than the other, and the narrowest was of a paler colour than the broadest side. The narrow side showed the usual structure of the true Pines in all the three principal sections, but the broad side in the transverse section possessed a greater degree of solidity than the narrowest side, and in both the longitudinal sections the vessels were filled with decussating fibres, and the discs were not only more sparingly bestowed but were also smaller and more obscure than those occurring in the other side. At the time this wood was examined he did not know whether it was a portion of a stem or a branch. He has since ascertained that it was a horizontal branch, and it then became interesting to determine whether the difference of structures was peculiar

to the piece of wood in question; whether it occurred in both the stem and branches of *Taxodium disticha*; whether it was peculiar to that kind of wood; or whether it was a general feature in the horizontal branches of other *Coniferæ*.

The first step in the investigation was to procure another branch of *Taxodium disticha*. This he did last summer, and marked the upper side before the branch was cut off. The structure of this branch agreed in every respect with that of the branch formerly examined, and the pale-coloured or narrowest side was the uppermost. The next step was to ascertain whether the stem of *Taxodium disticha* agreed in structure with the branches. For this purpose the author requested Mr. James Macnab, of the botanic garden of Edinburgh, to bring him from America a portion of a stem. This he was so kind as to do last winter. The stem was five inches and three tenths thick in the longest diameter. The pith was nearer one side than the other by three quarters of an inch. The surface of the cross section was of a uniform pale colour, with the exception of a spot surrounding the pith nearly an inch in diameter, of a slightly darker shade. On examining a number of sections of this stem, they were all found to agree with coniferous stems in general, and showed not a trace of the structure occurring in the under side of the horizontal branches.

Having thus ascertained that in *Taxodium disticha* the difference of structure alluded to was peculiar to the horizontal or nearly horizontal branches, the third step was to determine whether any other coniferous horizontal branches agreed in structure with those of *Taxodium disticha*. With this view Mr. Nicholas lately procured branches of ten different species of pines, and has found them all agreeing in structure with those of *Taxodium disticha*. The pith is always nearer the upper than the under side. The upper or pale portions have discs similar to those of the stems, and show no trace of decussating fibres in the vessels or spaces containing the discs. The under or darker-coloured portions have fewer, smaller, and more obscure discs than those contained in the upper part, and the spaces between the vertical partitions in both the longitudinal sections have decussating fibres, which, however, are often finer and more crowded than those in *Taxodium disticha*.

It may be right to remark, that in coniferous horizontal branches the pith is always more or less eccentric, and that in some instances the eccentricity is great. In a branch, for example, of the black spruce, the cross section, which is somewhat ovate, has a vertical diameter of three inches and three tenths. The distance of the pith from the upper side is only half an inch, and from the under side it is two inches and eight tenths. There are thirty distinct annual layers in the under side; but these thirty layers, when crowded into the space of half an inch in the upper side, are so minute that they can scarcely be enumerated. This, however, is an extreme case, the pith being in general less distant from the centre. The branches of some pines, particularly the larch, are nearly cylindrical, but even in these the pith is always out of the centre.

But although the upper and under sides of many, perhaps all, coniferous branches, present a different structure, yet such a difference is not entirely confined to the branches. In some few stems a similar difference has been seen in the opposite sides. In a stem of *Pinus Cedrus*, for instance, one of the sides was of a pale colour, and had the usual structure; the other side was of a darker colour, and had structure similar to that of the under side of horizontal branches. Another portion of the same kind of wood, however, was of a uniform colour, and had throughout the usual structure. A young stem of *Pinus laricia* had a structure similar to that of branches, and the same was observed in an upright stem of *Cupressus sempervirens.*

On the Formation of Wood. By Dr. West.

Dr. West exhibited a specimen of Bog Yew, in which, from the non-adherence of two successive annual layers, the central portion of the heartwood, though in close contact with the surrounding portion, which constituted the greatest part of the bulk of the tree, was throughout its whole extent perfectly distinct from it, so as to present the appearance of a small tree which had grown up through the centre of a large one, adapting itself completely to its cavity. He considered this singular phænomenon to be the result of a severe frost, which had either frozen a very thin layer of alburnum, so as to destroy its vitality, and thus prevent the next-formed layer from adhering to it, or else, without absolutely destroying it, had so affected its exterior surface, as to produce the same result. He expressed a doubt whether this exactly answered to the lesion called by the French *gelivure*; and produced a drawing, copied from one by Decandolle, of a section of a juniper tree affected with that lesion, in which the diseased layer was of comparatively considerable thickness, whereas in his specimen there was no appearance whatever of a diseased layer, however thin, nor any space where such could have been. He alluded also to another lesion, mentioned by Duhamel, called *roulure*, which consisted in the non-adherence of the annual layers, and so far appeared to have a greater resemblance to the case under consideration; but for want of a more detailed account he did not venture to pronounce whether they were identical. He next entered into the consideration of how far this case, and still more that of Decandolle's juniper tree, might be urged in favour of Duhamel's theory of the formation of wood, and against those of Decandolle and Du Petit Thouars; and remarked that at all events it clearly proved that the bark can form good wood, independently of the aid of the alburnum. He further adduced the fact, that the nodules of wood that are found on the trunk of the beech have always a layer of liber interposed between them and the alburnum; and expressed his opinion that this afforded an additional proof, that the bark has, in general, if not the sole, at least the predominant influence in the formation of wood. In this specimen, the annual layer formed after the occurrence, whatever it was,

that prevented its adhesion to that of the preceding year, was as thick and sound as any of those that were near it, though it must apparently have been formed wholly by the liber.

Notice of a Yew found in a Bog in Queen's County. By CHARLES WILLIAM HAMILTON, *Honorary Secretary of the Horticultural Society of Ireland.* (*Communicated by Mr.* MACKAY.)

In this tree Mr. Hamilton was able to count annual rings or layers indicating a growth of 545 years. Yet so compact was the wood, or so close the layers, that the diameter of the trunk did not exceed a foot and a half, or its circumference three feet and a half. The growth had been very slow during the last three centuries, for near the exterior there were about 100 rings within the space of one inch.

Many years ago Mr. Mackay measured a yew tree, growing on the island of Innisfallen on the Lower Lake of Killarney, of nearly double the dimensions of the one described by Mr. Hamilton, or between six and seven feet in circumference.

Notice of the Yew at Mucruss. By Dr. LITTON.

Dr. Litton had tried the age of the celebrated yew tree at Mucruss by Decandolle's test, and found that the result nearly agreed with the tradition. He exhibited a specimen of an oak tree bearing the impress of letters on the inner concave surface.

Mr. SAUNDERSON noticed a passage in an old Scotch history, which stated that the northern part of Ireland was so much infested by yew trees that a great emigration of Irish took place in consequence, who, with their families and cattle, went over to settle themselves in Scotland, the yew trees every year destroying their cattle in Ireland.

On Bog Timber. By the Rev. Archdeacon VIGNOLES.

The bogs of Westmeath are numerous, covering a considerable extent of the county. They almost invariably present the same natural appearance, only some are much more thickly imbedded with bog timber than others. In some of them there are three layers of trees to be found; and alternating with them as many layers of peat from three to five feet in depth. The trees in each layer appear to have arrived at maturity, and could not have been coexistent. The specimen of bark exhibited was taken from a tree 56 feet long; squaring from 2 feet to 18 inches: it lay upon a heathy bed; consequently where it fell the surface was heath. It was charred from top to bottom. With very few exceptions, all the timber found in the neighbourhood bears the marks of fire. The roots are rarely found attached to the tree, but likewise bear evident traces of having been burnt. They are of enormous size.

Dr. Martin Barry communicated the result of some observations on the colour of the sky, as seen from the summit of Mont Blanc; and expressed his conviction, that, while the depth of this colour appeared very much increased, as might be expected, from his elevated position, its peculiar tinge of black was in a great measure due to the contemporaneous reception by the eye of rays from the snow. He stated that the same effect has been observed by Boussingault in his attempted ascent of Chimborazo and other mountains.

Cursory Remarks upon some matters contained in a Letter addressed by Mr. William Hamilton to Mr. Packenham. By William Schiede, *M.D.* (*Translated by Mr.* Hamilton.)

1. The *Oxalis tuberosa* is a plant of Chili, not of Mexico; at least I have never heard of any plant of this genus with esculent roots being cultivated in the Mexican republic. The country abounds in wild species of *Oxalis* (the *Xoxocayallin* of Hernandez), some of which are applied to culinary purposes in the same manner as the sorrels (*Rumex*) of Europe.

2. The *Solanum tuberosum* is, without doubt, a native of this soil, as has been already published in the beginning of 1829. I have collected several varieties, which may, perhaps, prove to be distinct species. Moreover, I have collected among them one species (*Solanum oxycarpum*, Schiede) equally tuberous, and in every respect akin to the *S. tuberosum*, from which it differs in bearing pointed fruit. Notwithstanding which, the *Papa*, according to my researches, has no Aztec name, being known to the Aztecs by the name of *Papa*. Hernandez speaks of the Peruvian *Papa*; which proves how little he was aware of its being a plant of this country.

3. According to my observations, the *Cevadilla* is a new plant (*Veratrum officinale*, Schiede). Hernandez has described and figured it very indifferently under the name of *Hzcuinpatli*, or Dog-killer. It is a powerful anthelmintic, diuretic, antiarthritic, and antipsoric. I am not aware of its having been employed in the cure of hydrophobia. In the course of the last ten years a new species of *Veratrum* (*V. Orfilia*, Sabadilla,) has been published by Descourtilz, which he conjectures to be the plant which yields the cevadilla of the shops of Europe. In my opinion, this last plant is doubtful, and is at least distinct from the *Cevadilla* of this capital and of the shops of Berlin.

4. I am not acquainted with the plant called *Amole*, of the province of Sonora. A root is exposed for sale in the market-place of Mexico, under the name of *Amole*, which is the *Agave polyanthoides* of Schiede, or at least one nearly related to it. It is commonly used for washing linen, in place of soap, as it abounds in an extractive and saponaceous principle.

5. I do not know the *Cestrum Mutisii*. If I mistake not, it is a production of South America. In some parts of Mexico they employ in its place the sap of the *Justicia tinctoria*; but I cannot say whether or not its colour is as durable as that of the *Cestrum*, hav-

ing written a number of letters with it to Europe, which are consequently not in my possession.

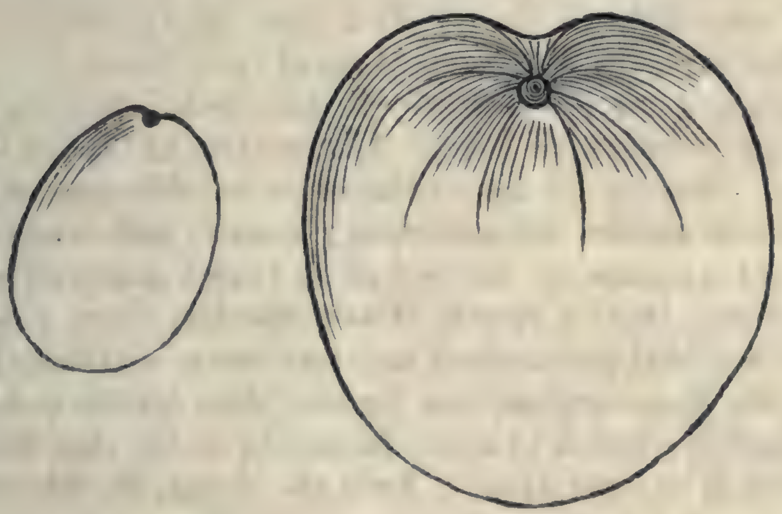
6. Should the *Huelosochil,* mentioned by Mr. Hamilton in his letter, be the *Yoloxochitl* of Hernandez, I can pronounce it to be the *Talauma Mexicana* of Jussieu. Its reputed medicinal properties are most probably exaggerated; and both the flowers and the seed are employed in this country in the cure of various nervous affections, and especially epilepsy. *Yoloxochitl* is an Aztec term implying the Flower of the Heart (*Flor del Corazon*).

I shall conclude these remarks by observing, that several of the matters indicated, together with a multitude of others relating to the vegetable kingdom of Mexico, have been discussed in a German botanical periodical: "Linnæa. Ein Journal für die Botanik in ihrem ganzen Umfange. Herausgegeben von D. F. L. v. Schlechtendal. Berlin, Jahrgang 1829, et seqq."

Mexico, 28th December, 1834.

Mr. Hamilton stated that he has lately received a pericarp and two nuts of the celebrated *Palo de Vaca,* from the vicinity of the farm of Barbula, spoken of by Humboldt; but, unfortunately, they did not reach him in a state fit for vegetation, and were apparently too old when gathered. He has written for a further supply of fresher fruit, and specimens of various ages, together with the flowers, which have never yet been botanically examined.

Below is a rude outline of the pericarp and one of the nuts, of their natural dimensions.



Pericarpii diameter equatorialis poll. 2; diam. polaris 1 $\frac{7}{8}$ poll.

He has also been favoured by Sir R. Ker Porter with a few seeds of the wax-tree of Guyana, and a candle made from its wax. A plant raised from one of these seeds is now growing in Pontey's nursery at Dublin, but has not yet assumed an arborescent character. What this tree is cannot yet be ascertained; it bears at present little resemblance to the genus *Amyris,* to which it might otherwise have been suspected to belong.

On the Formation of a Natural Arrangement of Plants for a Botanic Garden. By Mr. NIVEN.

The principal object of this plan is to divide the exotic from the European plants by a serpentine walk, bringing the allied species in juxtaposition by the numerous curvatures.

On Phænogamous Plants and Ferns indigenous to Ireland which are not found in England or Scotland. By Mr. MACKAY.

Mr. Mackay having been requested to present a general report on this and other branches of the botany of Ireland at the next meeting of the Association, this communication is omitted, as well as other notices of the same nature by Mr. Babington, Mr. Curtis, and Professor Graham.

Various other notices connected with the subjects of the papers were received from Dr. Coulter, Professor Graham, Mr. Curtis, Colonel Sykes, Mr. Fox, Mr. Waterhouse, Mr. J. B. Yates, Dr. Traill, Mr. Haliday, and Mr. Marshall.

MEDICAL SCIENCE.

On the Peculiarities of the Circulating Organs in Diving Animals. By JOHN HOUSTON, *M.D., M.R.I.A., &c. &c.*

The circulation of the fluids in living animals, though mainly carried on by the influence of the vital powers, is nevertheless to a certain extent amenable to the general laws of hydraulics. Gravity, motion of the particles of the solids upon each other, the respiratory function, pressure on the surface of the body, all, under various modifications, promote or retard the movement of the fluids along their vessels. But of all the collateral circumstances exerting an influence of this nature, the action of the chest and lungs appears, in warm-blooded animals, to be one of the most important. Suspension of respiration puts a stop to the circulation of the blood through the lungs; this fluid under such circumstances stagnates in the vessels leading to these organs, and cannot pass forwards until air be freely readmitted: death in a few moments is the inevitable consequence of such interruption. Animals living in atmospheric air cannot exist under a state of suspended respiration so long as those whose natural habitation is the water. The most expert diver has never been known to remain submersed for more than two minutes at a time, whilst it is well known that the whale can remain under water for upwards of twenty. Now, the arrangement of the respiratory and circulating organs in man and cetaceous animals, and the influence of these two systems on each other, being the same, though their powers of suspending respiration with impunity are very dissimilar, we naturally inquire, on what does this latter difference depend?

Independently of the suspension to respiration which occurs in

these animals while under water, there is another cause operating, when at great depths in the ocean, to the prejudice of their circulating fluids, such as is never experienced by terrestrial animals, namely, pressure on the surface of their bodies by the water, increasing with the depth from the surface. A boat, as observed by Scoresby, when dragged to the bottom of the sea by a whale, into which a harpoon was struck, became in a few minutes as completely soaked in every pore as if it had lain at the bottom of the sea since the flood: after being raised again to the surface, by the whale returning "to blow", it could with difficulty be got into the ship on account of its great weight; and a fragment of it, when thrown into the sea, sank to the bottom like a stone. And are we to suppose that a degree of pressure under water, sufficient to soak in an instant every pore in the planks of a large boat, was not felt by the animal which dragged it to such a depth? There can be little doubt that the application of this pressure would repel the fluids from the vessels near the surface of the animal into those more removed from its influence in the deeper recesses of its body; that, in fact, an effect would follow, the opposite of that which is produced in an animal when placed under the exhausted receiver of an air-pump; or such as occurs in persons attaining so high an elevation in the atmosphere as to be freed from some of its weight, in whom the blood is determined to the surface, producing giddiness, bleeding from the nose, ears, lungs, &c.

We may consider, therefore, that aquatic mammalia can exist with impunity during periods of suspended respiration, and also under degrees of pressure which would be destructive to the lives of animals of the same class whose element is exclusively the atmosphere. And this may be considered still more remarkable, when it is recollected that during those periods of breathlessness and universal pressure, the voluntary and rapid movements which these animals perform when in pursuit of their prey, tend to urge towards the lungs, where the principal obstruction exists, all the fluids contained in the veins among the muscular structures of the body.

The object of Dr. Houston's communication is to point out the provision on which these peculiar diving faculties of such animals depend; a provision beautifully harmonizing with all our physiological notions, and admirably adapted to the end in view. It consists of reservoirs connected with the veins leading to the lungs, where the blood may find a temporary resting-place during the period at which the asphyxiated condition of these organs refuses it transmission through the vessels. Dr. Houston exhibited numerous preparations and drawings demonstrating the presence of this singular provision in the porpoise, seal, otter, great northern diver, gannet, &c. The veins principally concerned in these dilatations are those nearest the heart, *viz.* the venæ cavæ, the venæ cavæ hepaticæ, the jugulars, the veins of the spine, and those in the posterior regions of the abdomen. In the seal the venæ hepaticæ form large bags in the liver; and in the same animal there is on the back, and along the sides, and posterior part of the neck, a plexus of veins of such size,

that, when they are filled with injection, the parts beneath cease to be visible; the vessels are as thick as the finger, and coiled, and heaped up on one another to an almost incredible amount. The contrast between the condition of the venous system in the great northern diver and that in the gannet, as exhibited by Dr. Houston, is important in establishing the uses of these reservoirs.

The diver and gannet are both seafaring birds, but differ remarkably in their modes of seizing the fish on which they feed. The diver swims under water after its prey, and remains at such periods long out of sight; the gannet pounces on it like an eagle, when discovered by its quick-sighted eye near the surface of the water, and thence carries it up to some dry spot, impaled on its long, sharp bill. As might be expected in those two birds of such opposite habits, the provision of reservoirs for stagnant venous blood is largely developed in the one, but completely absent in the other. In the diver the venæ cavæ and venæ cavæ hepaticæ are dilated to a size equal to that of the same veins in the adult human body, and there is, moreover, a kind of second auricle, designed to render the provision more complete; whilst in the gannet these veins, and all the others in the body, are of the ordinary dimensions.

Dr. Houston made allusion to the habits of pearl-divers, and offered a conjecture that in those individuals, to whom practice has given such a power of remaining long under water, some dilatation of the venæ cavæ and venæ cavæ hepaticæ may be gradually effectuated, giving them their superiority over other men in suspending the breath, and approximating them thereby somewhat to the condition of aquatic mammalia. The dilatations which are known to take place in these vessels in some varieties of disease of the heart, he adduced in evidence of the possibility of such an occurrence.

An Account of a Variety of Hydatid (Cysticercus tenuicollis) *found in the Omentum of an Axis Deer; with Observations on its Pathological Changes. By* JOHN HOUSTON, *M.D., M.R.I.A., &c.*

This hydatid, varying in size from an almond to an orange, generally single, sometimes in connexion with another, lies in a smooth membranous cyst between the layers of the omentum. Its head and body are in the living state inverted into the cavity of the caudal vesicle; but by immersion in tepid water they become visible, and are always found protruded and naked in hydatids which have undergone death before the decease of the parent animal. Dr. Houston considers that the inversion of the head is the natural condition, and that its eversion is the result of some irritation or of death. He also differs from most other helminthologists, in being of opinion that the lateral depressions on the head, termed mouths, and visible only to the microscope, are covered over with a thin pellicle, and incompetent, therefore, to the office assigned to them, *viz.* that of being agents for the imbibition of nutriments, as he found that fluids squeezed from the vesicle in the direction of the head, protruded and rendered convex the membranes of these aper-

tures before making its escape through them. Dr. Houston agrees in opinion with those who consider that the function of imbibition is carried on by the whole surface of the little animal. From the examination of the specimens of hydatids which existed in great number and variety in this case, the author has been enabled to describe and delineate the different stages of the process of degeneration, to which he considers all such animals are by their nature subjected; and has arrived at conclusions as to the seat of these degenerations different from those advanced by other authors. He considers, That the term allotted for the existence of each individual hydatid having expired, the little animal dies, and in the dead state comes to act as a foreign body on the cyst which contained it;—That the cyst, thus irritated, falls into a state of inflammation, the effects of which are traceable through a variety of stages, to the almost total disappearance of both cyst and hydatid. The cyst first becomes thickened; lymph is thrown out on its internal surface, giving it a roughened granular appearance. The hydatid becomes opake, and its fluid contents muddy. An adhesion, probably of a glutinous or mechanical nature, is established between the lymph and the exterior surface of the hydatid. The fluid of the hydatid is then absorbed, and its empty bag squeezed up in the centre of the solidified tumour. At a period somewhat later all traces of the hydatid disappear, and the remaining mass consists of nothing but the altered cyst, filled with lymph and some curdy matter. The tumour diminishes in bulk, it becomes of a cheesy consistence, and finally is converted into a small solid nucleus of earthy matter, devoid, as it would appear, of any irritating properties.

Dr. Houston differs from other writers in referring the whole of the morbid changes to the cyst, and not to the contained hydatid, which he says is absorbed in the progress of the phænomena consequent upon its death. He does not concur with those who are of opinion that malignant and tubercular diseases are of the nature of parasitical animals. No facts have hitherto been advanced sufficient to establish the position that any such diseases are, either at their commencement or at any subsequent period of their progress, of such a character. No animal has ever been seen of any definite shape in connexion with them; and where the powers of the microscope can be brought with such effect in aid of investigations of this nature, why, it may be asked, if such pestiferous animals exist, have they not ere this been demonstrated? The fact is, that all the circumstances connected with the growth and decay of such parasites as our senses can take cognisance of tend to a conclusion of an opposite nature, *viz.* that these animals have their periods of existence as living beings, and having passed from this state, instead of polluting the whole frame, or running into extensive diseases, disappear, and leave little or no injurious effects behind them, unless what may have arisen from their mechanical interference with the functions of some vital organ.

The author exhibited numerous preparations and drawings illustrative of the facts advanced in the paper.

On the Entozoa which are occasionally found in the Muscles of the Human Subject. By Professor Harrison.

The Professor exhibited preparations and drawings of a speckled appearance not unfrequently met with in different parts of the muscular system, and detailed the particulars of several cases in which it had existed: he expressed his full concurrence with the opinions advanced by Mr. Owen, in the Transactions of the Zoological Society of London*, as to the animal or vital character of the bodies to which the appearance is owing. He next remarked some interesting coincidences in the cases he had examined: thus, in one instance, where the muscles were very generally affected, he found a large cyst in the liver which contained several hydatids. These were exhibited to the meeting. In all the other cases there were marks of scrofulous disease having existed, either recently or at some remote period: thus, in three cases the lungs were a mass of tubercular matter, and in another there was caries of the lumbar vertebræ and scrofulous suppuration in the adjacent structures. The Professor further stated, that in all the cases he had examined, this appearance was almost confined to the voluntary muscles: he had never met with it in the heart or intestinal tunics, but had found it about the circumference only of the diaphragm, and in the other mixed muscles to a much less degree than in the voluntary: these bodies he stated to be more numerous on the cutaneous than on the deep surfaces of muscles, and to be deposited in the interfascicular cellular tissue, rather than in the fasciculi themselves.

On the Bones which are found in the Hearts of certain Ruminant Animals. By Professor Harrison.

The author first compared the circulating organs in fish, reptiles, birds, and mammalia. He next adverted to the opinions of Morgagni, Haller, Daubenton, Meckel, and Carus as to the singular osseous appendages which the hearts of some of the ruminants possess, as also of some other animals allied to them. He exhibited several specimens of these bones, some dried, some in their recent state, and others *in situ*, in different animals. The heart of the ox presents them in greatest perfection; here there are always at least two, and sometimes several smaller osseous and cartilaginous grains: the two principal bones are, one very large, placed posteriorly in the septum auricularum; the other, smaller, is situated in front. The large one is of the figure of the human malar bone; its upper concave border forms the floor to the posterior aortic sinus; its inferior bevelled edge gives attachment to the large portion of the mitral valve; to the body of the bone the fleshy and tendinous fibres of the auricles are attached. The small or anterior bone is triangular; its concave base floors the anterior aortic sinus. These bones are always to be found in both sexes, and in the young as well as in the old. Specimens were presented from animals only a few weeks old, in which

* An abstract of Mr. Owen's paper on the *Trichina*, here referred to, was given in our last volume (vi.), p. 452.—Edit.

osseous nuclei were distinct in the cartilaginous basis. The author next adverted to the peculiar fleshy character of the left ventricle in the ox, a transverse section exhibiting the appearance of a puncture or stab, rather than of a distinct chamber; this formed a curious contrast to the heart of the horse. From these and many other observations, Mr. Harrison inferred that these bones are supports, not only to this mass of muscle, but also to the root of the aorta which is connected to them, and which is thus maintained in a permanently open state; while, again, there being two bones connected by ligament, the elasticity of the vessel is not impaired. These bones, moreover, serve to support the septum of the auricles, and to prevent their perfect closure or collapse, and they also floor and support two of the aortic sinuses with their semilunar valves. The Professor next spoke of the peculiar, hard, marble-like fat which is deposited in masses about the roots of the great arteries, and showed that these cover the three sinuses of the pulmonary artery, and that sinus of the aorta which is deprived of osseous support. These arterial sinuses are lodged in excavations in the fatty deposits alluded to, and no ordinary force can overcome the resistance which they offer to overdistension; and thus the sinuses are enabled to support the returning columns of blood, which are impelled by the elasticity or resiliency of the arteries, which in such animals are peculiarly strong and elastic. The author next explained the structure and true use of the corpora Arantii; contrasted the structure of the pulmonary artery and its valves with the corresponding parts of the aorta; and concluded with some observations on the calcareous and osseous deposits which are met with in the human subject, in whom they appear as accidental or morbid changes in those very situations where in some animals the osseous structure is essential.

On the Structure of the Mammary Glands in the Cetacea; with observations on the Mechanism of the Mouth and soft Palate, as applied by the young Animal in Sucking. By A. Jacob, *M.D., Professor of Anatomy, Royal College of Surgeons, Ireland.*

The author, commenting on the opinions of M. G. St. Hilaire in his work entitled "Fragmens sur la Structure et les Usages des Glandes mammaires des Cetacés," and referring to the descriptions of Hunter and plates of Müller, entered into the question of the mechanism of the mammary glands in Cetacea, and the operation of the mouth of the young of that tribe.

M. St. Hilaire is stated by the author to entertain the opinion that "the process of nutrition of the young of the Cetacea by the milk of the mother, is accomplished in a manner and under circumstances different from those of other mammalia." To support this proposition, M. St. Hilaire assumes that the mammary glands in these animals are peculiarly organized and circumstanced; first, in being placed between the abdominal and subcutaneous muscles, by which they are subjected to mechanical pressure adequate to the expulsion of their contents; and secondly, in containing a peculiar reservoir,

formed by an enlargement of the excretory ducts, and running the whole length of the organ.

These statements are admitted by Dr. Jacob; but he remarks, that there is no proof of any *special pressure* on the mammary gland arising from its position with reference to the muscles; and that the only *peculiarity* in the excretory ducts is the existence of the mammary reservoir, in the form of a single cavity,—a circumstance which the author considers to be dependent on the flat, elongated form of the mammary gland. He advances arguments to show the probability of there being, in fact, a special structure at the orifice of the nipple to *prevent* loss of milk by any other external pressure than that upon the teat or nipple itself.

Both M. St. Hilaire and Mr. Hunter have assumed that, in consequence of the opposite condition of the nostrils of the mother and young during the act of suction, this process can only be performed by the young between two respirations. The act of sucking, Mr. Hunter states, must also be different in the Cetacea from that of land animals, "the lungs having, in the former, no connexion with the mouth." On these points the author differs from the eminent authorities quoted, and enters into an examination of the action of the soft palate in the functions of breathing and deglutition; from which he deduces the conclusion, that *the mouth is a separate and distinct cavity, capable of increasing or diminishing its capacity, and, consequently, of forming an imperfect vacuum, into which the milk rushes in sucking, and from which, when accumulated, it is transferred to the œsophagus.* It must not be forgotten that the construction of the soft palate in the Cetacea is different from that in other animals: it is in them in the shape of a muscular partition, with a circular aperture surrounded by a sphincter; while the top of the larynx is elongated so much upwards that it enters this aperture, and, being grasped by the sphincter, communicates with the blow-hole or nostril, leaving the mouth and fauces unaffected by the process of respiration, and still better adapted than in other animals to carry on the operation of sucking.

On the Mechanism of Bruit de Soufflet. *By Dr.* Corrigan.

The first part of the paper consisted of an analysis of the various theories which had been proposed to account for this sound and its varieties, *bruit de rape*, &c. Laennec supposed it to be produced by spasmodic action, but his opinion has been generally abandoned. By some the sound has been attributed to increased pressure made by narrowing of the heart or arteries,—but it is heard in permanent patency of the aorta, in the vessels of the pregnant uterus, in aneurismal dilatation of arteries in varicose tumours, in all which instances there is no narrowing;—by others to increased velocity in the motion of the blood; but it is not heard in the circulation of the fœtus or infant, while it is audible in the slower circulation of the mother; nor in the quickened pulse of hectic or inflammatory fever, while it is audible with a pulse of 70. By others it is attributed to rough-

nesses in the interior of arteries, or irregularities, over which the blood, in passing, produces the sound; but it is not heard in the healthy heart, the internal surface of which is exceedingly irregular; nor is it necessarily present in aneurisms, rough and irregular on their inner surface, from shape, or from deposition of fibrine; the sound, on the contrary, being frequently heard when there is no deviation from the natural state of the interior surfaces of the heart or arteries.

The second part of the paper developed Dr. Corrigan's views. His theory is, that the sound depends on the simultaneous presence of these two conditions, viz. 1st, a current-like motion of the blood (instead of its natural equable movement), tending to produce corresponding vibrations on the sides of the cavities or arteries through which it is moving; and, 2ndly, a state of the arteries or cavities themselves by which, instead of being kept in a state of tense approximation on their contained inelastic blood (which would necessarily prevent any vibration of their sides), they become free to vibrate to the play of the currents within on their parietes; and by those vibrations cause, on the sense of touch, "*fremissement*," and on the sense of hearing, "*bruit de soufflet*." It was shown that these two conditions are present in the parietes of the ventricle, and the currents of blood striking against them in cases of narrowed auriculo-ventricular openings; in the enlarged and tortuous arteries of the placental portion of the uterus permitted by their very free anastomosis with veins and sinuses, and other causes, to become partially flaccid in the intervals of the heart's contractions, and the irregular currents necessarily assumed by the blood in rushing along these comparatively flaccid tubes at their next diastole; and that similar conditions exist in the analogous state of the vessels in aneurismal dilatations of tortuous arteries. The presence of the two conditions was also applied to explain the mechanism of the sound in permanent patency of the mouth of the aorta, in the large arteries of animals dying of hæmorrhage, and in various other instances. In conclusion, two experiments were detailed, in which, in one instance, a small bladder, and in the other a portion of the gut of an animal, was interposed between two cocks, the upper or nearer being the cock of a water-cistern, and the lower or further constituting the discharging orifice of the bladder or gut, and water then allowed to flow through from the cistern. The sound "*bruit de soufflet*," and the sensation "*fremissement*," were perceptible in the intervening bladder or gut, until (from the upper pipe pouring in fluid faster than the lower discharged it) the bladder or gut became tense, and then both sensations ceased, the passage of the fluid through, nevertheless, continuing all the time. The experiment with the bladder was applied to explain the occasional presence and absence of "*bruit de soufflet*" in aneurisms, the sound being present in an aneurism when, from any circumstance connected with it, its parietes can become at all flaccid in the intervals of the heart's contractions,—not being heard if the parietes remain tensely applied to their contained fluid.

Dr. Corrigan has in some experiments substituted a gum-elastic tube for the portion of gut.

Dr. Alison read a notice of a few experiments and observations which he had made, with the assistance of different friends, on two distinct subjects: 1. On the Condition and the vital Powers in Arteries leading to inflamed parts, (in continuation of those on the same subject read to the Section in 1834); and 2. On the immediate Cause of Death in Asphyxia.

He connected them with one another by some preliminary observations on the importance of establishing the truth, and, as far as posssible, determining the applications of the principle to which the term *spontaneity of movement* in the fluids of living bodies has been applied, *i. e.* of movements of the fluids in living bodies, which are dependent on their living state, but independent of any contraction of their living solids.

In proof of the truth of this principle he stated that many facts might be adduced; and the immediate object of the statements now made was to prove that without reference to this principle it is impossible to explain two sets of phænomena, which have been carefully observed, and are of essential importance,—the changes in the motion of the blood which attend inflammation, and those which result from the application of oxygen to the blood in respiration.

On the first point, he detailed the result of two examinations (in addition to those formerly reported) of the arteries of limbs of horses killed on account of injury and inflammation of single joints, in one case of three weeks', in the other of eight days' standing. The power of contracting on a distending force, and expelling their contents, was tried in the arteries both of the inflamed and the sound limbs, by the same contrivance as was used by Porseuille to compare the contractile power of living and dead arteries; *i. e.* by using bent tubes and stopcocks in such a way as to distend a given portion of artery (first of the one limb and then of the other,) by water pressed into it by a firm weight of mercury, and then allowing the artery to expel the distending water, and getting a measure of the force which it exerts in doing so, by the rise of the level of water in a tube communicating with the artery. The result was in both cases in accordance with the observations formerly made, that the artery of the inflamed limb exerted *less* power of contracting on, and expelling its contents, than that of the sound limb. The difference was as 10 to 16 in one case, and as 125 to 175 in the other, which was the more satisfactory of the two, as the experiment was made more immeately after death.

It appeared also, on careful comparative examination, that the contraction of the emptied arteries at the moment of death (which is the measure adopted by Parry of the vital power of arteries) was less in the diseased than in the sound limbs; the difference between the contracted state immediately after death, and the subsequently dilated and dead state of the artery (28 hours after death), being $\frac{1}{4}$th in the case of the diseased limb, and $\frac{1}{3}$rd in that of the sound limb.

It appears, therefore, that in all arteries of such size as to admit of measurement, and which supply inflamed parts, the only vital powers of contraction, which experiments authorize our ascribing to the coats of these vessels, is *diminished* during inflammation; and it may be safely added, that no other change but this diminution or relaxation of contractile power has ever been perceived, either in them, or in the smaller vessels which come under the observation of the microscope, at least during the greater part, and in the highest intensity, of inflammation.

But if it be inferred from these facts that inflammation consists merely in relaxation of vessels, giving an increased effect to the impulse of blood from the heart to the part affected, several facts may be stated to show that the explanation thus afforded is quite inadequate. The change which takes place on the movement of the blood flowing to an inflamed part is, diminution of velocity or absolute stagnation in the vessels most affected, combined with increased velocity and increased transmission in all the neighbouring vessels; and it seems impossible to ascribe both these opposite effects to the same cause, viz. a simple relaxation or loss of power in the vessels concerned. Neither can the characteristic effusions consequent on inflammation, and by which alone it is uniformly distinguishable from simple congestion or serous effusion, (and particularly the increased quantity and increased aggregation of the fibrin that exudes from inflamed vessels,) be explained by this change of the action of the vessels. And further, the local causes which excite inflammation are not only such as in other instances produce an increase, instead of a diminution, of vital power, but they are such as have been ascertained to produce, when they are made to act on minute portions of individual vessels only, contraction instead of relaxation; as has appeared in the experiments of Verschuir, Thomson, Hastings, Wedmeyer, and others.

The proper inference, therefore, appears to be, that the idea of an *increased action of vessels* in an inflamed part is indeed a delusion; but that there is a really *increased action within the vessels* of the part, *i.e.* an increased exertion of powers, by which the motion of the blood is affected, but the action of which is independent of the contractions of the living solids, and the effect of which is to cause distention and relaxation of the vessels, within which they act with unusual energy.

2. The immediate object of the experiments on death by asphyxia was to ascertain whether the acceleration of the flowing blood through the lungs,—which is undoubtedly produced by respiration, and the failure of which appears, from the experiments of Williams of Liverpool, and of Kay of Manchester, to be the immediate cause of death by asphyxia, can be ascribed, as Haller and some very recent authors have supposed, to the merely mechanical influence of the alternate expansion and contraction of the lungs by the respiratory movements.

That this is not the fact might be concluded from the fatal asphyxia produced by breathing azote or other gases, not poisonous,

but not containing oxygen; in which case it had been observed by Broughton and others, that the stagnation of blood in the lungs, and the distention of the right side of the heart, take place equally as when the respiratory movements are suspended. But to this observation it might be objected, that the animals on which experiment had been made had been allowed to remain in the azote until they became insensible, and their respiration of course ceased, and had not been examined until some minutes after their apparent death, and it might be said, that the right side of the heart had become congested only after the acts of respiration had ceased, and in consequence of their cessation.

In order to avoid this source of fallacy several rabbits were confined in azote, only until their breathing became laboured, the respirations generally less frequent, but much longer and fuller than natural. They were then taken out and instantly struck on the head with such force as to crush the brain and cerebellum, and arrest the circulation as instantaneously as possible. This was always attended with violent and general convulsion, but with no attempt at respiration, sensation being apparently instantaneously suppressed. When the body was opened immediately after the convulsion had subsided, the right side of the heart was always found distended with blood, and palpitating feebly; the left side at rest and comparatively empty: the quantity of blood obtained by puncturing and pressing the right side and pulmonary artery was from 5 to 10 times as much as could be obtained from the left side and aorta. When a rabbit previously breathing naturally was killed in the same manner, the quantity of blood on the right side of the heart (apparently accumulating there during the convulsions) was found to be greater than on the left; but the difference was decidedly less than when it had been breathing azote; and in one of these comparative trials the blood in the left side was found to be sufficient to keep up a feeble palpitation in that side, whereas in the animals that had breathed azote the left side was always found quite at rest.

It appears from these experiments that when oxygen is not admitted into the lungs in inspiration, even although the respiratory movements continued further and more forcible than usual up to the moment of death, the blood stagnates on the right side of the heart; and that the application of oxygen to the blood at the lungs is a cause of acceleration of its movement through the lungs, independently of any influence of the mechanical movements of respiration.

If we further inquire, in what manner oxygen can give this stimulus to the flowing blood through the lungs, it appears certain that it cannot be by stimulating the small capillaries of the lungs (the only vessels to which it is directly applied) to contraction, because even if it be granted that there are vessels capable of contracting on irritation (which is very doubtful), the immediate effect of stimulating any arteries capable of taking on such action has always been observed to be a constriction permanent for some length of time, and in consequence a *retarded* flow of the fluids through them, as in the experiments of Wedmeyer.

If, again, we suppose the effect of the oxygen on the minute ves-

sels in the lungs to be sedative or relaxing, and ascribe to a diminished action of these vessels the apparently increased efficiency of the right side of the heart when oxygen is applied, we suppose the oxygen to produce the very opposite effect to that which has always been observed when it or any other stimulus has taken effect on any individual artery.

The only mode in which it appears possible to escape from these difficulties is to suppose that the stimulus given by the oxygen to the flowing blood through the lungs, is a stimulus to that movement which is independent of any contraction of the solids containing the blood. This conclusion is in perfect accordance with the observations of Haller on the *derivation* of blood, perceptible under the microscope, towards any part where an opening is made in a vessel, and air admitted into contact with the blood, because he gives satisfactory reasons for thinking that this derivation is not owing to contraction of the vessels; it is also in accordance with observations on some of the lowest tribes of animals, and on vegetables, where *currents in fluids* are observed in connexion with the act of respiration, but no movement of solids has been detected; and even, as Dr. Alison thinks, with the observations of Purkinje and others, on currents connected with the respiratory organs in animals much higher in the scale, because although these last currents have been ascribed by most authors to vibrations of ciliæ, which are seen to accompany them in various instances, it seems very doubtful whether they can be adequately explained without supposing a "*jeu d'attraction et repulsion*" to be commenced in these instances, as well as in the respiration of the lowest tribes.

[To be continued.]

GEOLOGICAL SOCIETY.

June 10.—A paper was first read, entitled "Note on the Trappean Rocks associated with the (New) Red Sandstone of Devonshire;" by Henry T. De la Beche, For. Sec. G.S.

The author remarks, that while trappean rocks are not found among the (new) red sandstone series of Somersetshire and the more northern portions of Devonshire, the southern portions of the latter county afford many examples of the association of trappean rocks with the lower parts of this series, particularly near Tiverton, Thorverton, Silverton, Kellerton Park, Crediton, and Exeter.

When hastily viewed, the trappean rocks might be mistaken for masses of igneous matter which have been intruded, in a state of fusion, among the beds of red sandstone. A more detailed examination of the various facts connected with their mode of occurrence leads, however, in the opinion of the author, to the inference that they have been produced by volcanic action during the formation of the lower parts of the (new) red sandstone series; in fact, that the trappean rocks in question are the remains of melted rock, either ejected from, or retained within the pipes of, volcanoes which were in a state of activity

during the production of the lower part of the (new) red sandstone series of Devonshire*.

The author endeavours, in the first place, to point out the relative geological age of the red sandstones and conglomerates with which these trappean rocks are associated in Southern Devon, by showing, that in their continuation to the northward, along the skirts of the grauwacke to the shores of the Bristol Channel, they pass into a series of beds which is crowned by magnesian limestone and conglomerate, equivalent to the magnesian limestone and conglomerate of the Mendip Hills and the vicinity of Bristol. The beds beneath the magnesian conglomerate, which very rarely passes into a magnesian limestone, from the absence of pebbles derived from older rocks, consist, for the most part, of red or claret-coloured sandstones, with an occasional seam or bed of conglomerate, the cementing matter of which is not calcareo-magnesian. Their thickness is necessarily variable, from the uneven surface of grauwacke, upon which the sandstones rest unconformably; but it amounts to about one hundred and fifty feet in the vicinity of Wiveliscombe. The author points out, by the aid of sections, that the magnesian conglomerate may readily rest upon the grauwacke, and conceal the lower red sandstone series by overlapping it, and that therefore it becomes exceedingly difficult to obtain an average thickness of these lower red sandstones, which, if we consider the magnesian conglomerates of the Mendip Hills as an equivalent of the zechstein of Germany, would be equivalent to the *rothe todte liegende* of the same part of Europe, and therefore be of the same geological age as the lower red sandstones of the North of England described by Prof. Sedgwick, and the beds noticed by Mr. Murchison.

Having thus obtained the relative geological age of the beds with which the trappean rocks are associated, the author proceeds to point out the occurrence of beds of sand among the more common red sandstone, which presents every appearance of having been volcanic sands ejected from a crater, and which became subsequently mixed with common detrital matter then depositing. It is stated that though the trappean rocks may sometimes be seen, as in the vicinity of Exeter, to rest as if they had overflowed the grauwacke which the (new) red sandstone series skirts, they are generally separated from the grauwacke by conglomerates, or sandstones, which do not contain the detrital remains of trappean rocks. Hence the author considers that the (new) red sandstone series of the district generally was, to a certain extent, in the course of formation when the volcanoes came into activity*.

A description is given, accompanied by a section, of the manner in which the trappean rocks of Washfield, near Tiverton, are associated with the (new) red sandstone of the same locality, and it is inferred from the facts detailed, that events there succeeded each other in the following order: 1. An original subaqueous valley or depression in the grauwacke. 2. A deposit of detrital matter derived from the

* [See note on next page.—Edit.]

subjacent grauwacke. 3. An eruption of igneous substances, in the manner of modern volcanoes, beneath very moderate pressure. 4. The deposit of detrital matter, in a great measure derived from the neighbouring grauwacke, mingled with fragments of trappean rocks, many of which may have been ejected, as fragments now frequently are, from volcanic craters. 5. Denudations at various geological epochs since the period of the (new) red sandstone, which have left the rocks as we now find them.

It is noticed as a fact, which the author conceives to be of difficult explanation without the aid of this volcanic hypothesis, that in the localities where the trappean rocks, associated with the red sandstone, occur, there are numerous angular fragments, some of considerable magnitude, even equal to one or two tons in weight, intermingled with the conglomerates, which do not resemble any trappean rocks discovered, in place, within the district. These fragments principally consist of reddish brown quartziferous porphyry, the base being felspathic, and the contained crystals being those of quartz and glassy felspar, the latter often attaining a large size. Though quartziferous porphyry is observable in place in some situations, as, for instance, to the northward of Dunchideock, near Exeter, it does not contain the large crystals observable in numerous fragments of porphyry included in the red conglomerates. The author, therefore, is inclined to consider, that these angular fragments have been ejected from volcanic vents, and that, falling upon the detrital matter then in the course of deposition around such vents, they became included among it. It is remarked that these fragments, as well as those of the more common, scoriaceous, and other trappean rocks, found in place, do not form component parts of the red conglomerates beyond somewhat moderate distances, measured from situations where the existence of volcanic vents, during the early part of the (new) red sandstone epoch, may be considered a probable inference, from the various observed phænomena*.

A memoir was next read "On the range of the Carboniferous Limestone flanking the primary Cumbrian Mountains; and on the Coal-

* [The geological reader may compare, perhaps with some advantage, the inferences stated by Mr. De la Beche in the very interesting memoir noticed above, with a paper on the consolidation of the new-red-sandstone, published in the Phil. Mag. and Annals, N. S., vol. vi. p. 71—75. The views offered in that paper were suggested to me, in part, by certain phænomena of the association of trappean rocks with the new-red-sandstone in Devonshire, as exhibited principally at Kellerton and Thorverton (and many of which are now described by Mr. De la Beche,) and by analogous cases in the *rothe todte liegende* of the Continent of Europe, as described by various foreign geologists. An error which I committed on another subject, connected indirectly with this, and mentioned in an accompanying notice (Phil. Mag. and Annals, N. S., vol. vi. p. 75—76), was corrected by Mr. Featherstonhaugh, in the succeeding volume (vii. p. 198—201); but the remark of that geologist, that "any desiccated salt lake" might be expected to contain the peculiar aggregates of crystals of chloride of sodium, the production of which I had ascribed to the agency of heat,

fields of the N.W. Coast of Cumberland, &c.;" by the Rev. Adam Sedgwick, F.G.S., Woodwardian Professor in the University of Cambridge, and Williamson Peile, Esq., F.G.S., of Whitehaven. An abstract of this paper appears in the "Proceedings" of the Society, No. 41.

A paper was afterwards read, "On the occurrence near Shrewsbury of Marine Shells of existing species in transported Gravel and Sand, resting upon a peat bog which contains imbedded Trees;" by Joshua Trimmer, Esq., F.G.S.

In November of last year Mr. Trimmer noticed, that in widening the road about five miles from Shrewsbury, towards Shiffnal, some very black timber was extracted from beneath a bed of loam and gravel; and having subsequently examined the spot, he has communicated his observations in this paper.

Two sections have been cut, to the depth of 15 feet, and are distant from each other about 600 yards. The eastern excavation is 360 yards long, and consists, proceeding from east to west, of 200 yards of sandy loam and gravel; 40 yards of sand resembling sea-sand, the laminæ crossing each other in various directions; 60 yards of reddish loam, with curved laminæ near its junction with the sand, and horizontal at the lower part, the upper portion not being laminated; and lastly, 60 yards of sandy loam and gravel. Fragments of shells occur in every part of the section, but are most abundant in the veins of fine gravel which pervade the sand: among them the author found *Turritella terebra, Cardium edule,* and *Tellina solidula.*

The western excavation contained fewer shells, and presented near the eastern termination of the southern side: cultivated soil, 1 foot; whitish and reddish finely laminated loam, 6 to 8 feet; peat, with prostrate trunks of oak trees, 6 inches to 2 feet; black clay, 4 inches; whitish sandy gravel, 12 to 18 inches, passing beneath the level of the road into reddish sandy gravel. Still nearer the eastern termination, the section presented thin beds of laminated loam and sand resting on peat. On the southern side this excavation consisted of fine cultivated soil, 1 to 2 feet; sandy loam, with occasionally boulders of several varieties of granite, some more than 2 feet long, and patches of peat, containing fragments of oak, beech, and fir, 6 feet; blackish loam, enveloping the upper part of a fir-tree, 6 inches; peat, enveloping the lower part of the fir-tree, 2 feet: the base of this tree was not visible, nor had any trees still rooted been noticed by the workmen. The patches of peat in the bed of loam the author conceives were derived from the tearing up of part of the peat bog.

From these details the following changes are inferred:

1st, A surface of dry land, consisting of gravel derived from the neighbouring rocks, either while the district was submarine, or during

is contrary to fact; and the arguments respecting the mode of consolidation of the rock exhibiting them, which I had deduced from their occurrence, and which will probably be found to be equally applicable to the history of the true new-red-sandstone, in certain localities, remain unaffected by the facts and reasoning which Mr. Featherstonhaugh brought forward.—E. W. B.]

the rise of the strata, or by subsequent denudation, or by these causes united.

2ndly, The surface was covered with a forest of birch, oak, and fir.

3rdly, The forest was destroyed, or it decayed, and a peat bog was formed.

4thly, A rush of sea buried the bog beneath a mass of loam and gravel, containing fragments of existing marine shells and granite boulders.

In conclusion, the author draws attention to the natural sections on both sides of the Severn, west of Shrewsbury, about one mile above the Welsh Bridge, in one of which he obtained, after much search, a few fragments of shells; and he begs geologists in general, both in England and Ireland, to institute a patient examination of the superficial gravel of their neighbourhood for fragments of shells, however, comminuted.

A paper was also read, entitled, "Description of some Fossil Crustacea and Radiata;" by William John Broderip, Esq., F.G.S. F.R.S., &c.

Lord Cole and Sir Philip Egerton having placed in the author's hands some fossils which they had lately found in the lias at Lyme Regis, a detailed account is given, in the memoir, of those which he considers to be new.

Crustacea.—The first specimen described consists of the anterior parts of a macrourous Decapod, between Palinurus and the Shrimp family, but of a comparatively gigantic race; and its organization being considered by the author to be *sui generis*, he has assigned to the fossil the name of *Coleia antiqua*, with generic characters which are given in the "Proceedings".

The collection contained the remains of other macrourous Decapods. One of these specimens consisted of a fragment of the post-abdomen, approaching nearest in sculpture to Palinurus, and equaling in size the sea crawfish: and two others are peculiarly interesting from their exhibiting the tips of the four larger branchiæ, and of the four smaller ones below, pointing towards the situation of the heart, and proving, the author observes, that this Crustacean did not belong to the Amphipoda, but to the highest division of the Macroura, of the arctic forms of which it reminds the observer.

Radiata. *Ophiura Egertoni*.—This species, Mr. Broderip states, approaches very nearly to the recent *Ophiura texturata*, and differs from *Ophiura Milleri* of Phillips, in as much as, among other differences, the disk of the latter is lobated according to the figure given in the "Geology of the Yorkshire Coast." The specimens were found about half a mile west of Bridport harbour, in masses of micaceous sandstone fallen from the cliffs.—*Cidaris Bechei*.

A letter from Sir Philip de Malpas Grey Egerton, Bart., M.P., V.P.G.S., addressed to the President, "On the Discovery of Ichthyolites in the South-western Portion of the North Staffordshire Coal-field," was then read.

These ichthyolites consist of teeth, palatal bones, and scales, belonging to the Placoidian order, and to the Sauroid and Lepidoidian

families of the Ganoidian order of M. Agassiz*. Some of the scales correspond precisely with those of the Megalichthys, described by Dr. Hibbert, from Burdiehouse near Edinburgh: but the plants associated with the ichthyolites, the author states on the authority of Professor Lindley, are entirely dissimilar from those found at Burdiehouse. Further particulars are given in the "Proceedings".

A paper was next read, "On the Bones of Birds from the Strata of Tilgate Forest in Sussex;" by Gideon Mantell, Esq., F.G.S.

Mr. Mantell states that soon after his attention was first directed to the fossils of the Wealden, he discovered in the strata of Tilgate Forest several slender bones, which, from their close resemblance to the tarso-metatarsal bones of certain Grallæ or Waders, he was induced to refer to birds. The correctness of this opinion was afterwards doubted, in consequence of the thin fragile bones found at Stonesfield, and considered as belonging to birds, being ascertained to be those of Pterodactyles. Having subsequently discovered a few specimens of more decided character, Mr. Mantell submitted them to the inspection of Baron Cuvier, during his last visit to England, who pronounced them to belong to a Wader, probably to a species of Ardea. Still it was doubted whether these remains did really belong to those of birds; but the author's attention having recently been directed to the subject, he placed his specimens in the hands of Mr. Owen, of the College of Surgeons. This gentleman, after a careful examination, pointed out that one bone decidedly belonged to a Wader, being undoubtedly the distal extremity of a left tarso-metatarsal bone, presenting the articular surface or place of attachment of the posterior or opposable toe. Other specimens of long bones Mr. Owen conceives may have belonged to a more erpetoid form of bird than is now known. From this examination, Mr. Mantell's previous views of the existence of birds below the chalk have been fully established, and, as the author observes, these are the oldest remains of the class at present known. The memoir concludes with a description of the bones, consisting of a tarso-metatarsal of a Wader, a tibia (?), a metatarsal bone, a humerus, and an ulna.

The next paper read was entitled, "Remarks on the Coffin-bone (distal phalangeal) of a Horse, from the Shingle Bed of the Newer Pliocene Strata of the Cliffs near Brighton;" by Gideon Mantell, Esq., F.G.S., &c., a notice of whica is given in the "Proceedings".

An extract was lastly read, of a letter from Dr. Daubeny, in which he gives the analysis of the mineral spring lately discovered near Oxford, and announced to the Society by Dr. Buckland, at the Meeting held on the 29th of April, stating that the water at the time the analysis was made (March 26th) contained more sulphuric salts than any other spring in this country; a pint containing 132·87 grains of solid contents, entirely saline, of which 52·40 are sulphate of soda.

This being the last Meeting of the Session, the Society adjourned, at its close, to Wednesday, November 4th.

* [A sketch of the classification of fishes, established by M. Agassiz, will be found in the Lond. and Edinb. Phil. Mag., vol. v. p. 459.—Edit.]

LINNÆAN SOCIETY.

Nov. 3, 1835.—Read Descriptions of some new species of *Diopsis*. By John O. Westwood, Esq., F.L.S.

This forms a supplement to the very interesting monograph of that curious genus of insects from the pen of the same author, which is printed in the last part of the Society's Transactions. The names and characters of the new species are as follows:

22. *D. Wiedemanni*, capite medioque abdominis rufescentibus, thorace nigro, spinis 2 scutellaribus 4que thoracicis flavidis, alis fuscescentibus in medio obscurioribus: maculâ ante apicem sublunari. Long. corp. lin. 4. *Habitat* in Guineâ, Africæ. In Mus. Wiedemann.
23. *D. erythrocephala* (Klug, MSS.), capite lætè ochraceo, pedunculis oculiferis obscurioribus, thorace nigro, pedibus anticis pallidè luteis tibiis tarsisque fuscis, alis pallidè fuscescentibus: maculâ ante apicem transversâ. Long. corp. lin. 3¼. Expans. alar. lin. 5½. *Habitat* in Promontorio Bonæ Spei. *D. Lichtenstein.* In Mus. Reg. Berolin.
24. *D. arabica*, capite pallidè fulvo, pedunculis oculiferis obscurioribus, thorace nigro, collari luteo-fulvescenti, tibiis anticis posticisque fuscescentibus. Long. corp. lin. 3. *Habitat* in Arabiâ Desertâ. *D. Ehrenberg.* In Mus. Reg. Berolin.
25. *D. Miegenii* (Wied. MSS.), nigra; pedunculis oculiferis spinisque scutellaribus fuscis, abdomine ad basin fasciis duabus (posticâ interruptâ) argenteis, alis maculâ parvâ centrali fasciâque angustâ fuscescentibus. *Habitat* in Guineâ. In Muss. Reg. Berolin et Wiedemann.
26. *D. Neesii*, capite rufescenti, thorace obscurè nigricanti, scutello pallidiori, abdominis basi rufâ apiceque nigro, alis 3-fasciatis. *Habitat* ——. In Mus. Acad. Bonnensis.

The commencement of a paper, entitled, "Descriptions of Indian *Gentianeæ*," by Mr. David Don, Libr. L.S., was also read.

ZOOLOGICAL SOCIETY.

September 22.—The reading was concluded of an anatomical description, by Mr. Reid, of the *Patagonian Penguin, Aptenodytes Patachonica*, Forst.

"The specimen, an adult male, whose dissection forms the subject of the following paper, was captured at East Falkland Isle, in latitude 51° 32′ south, by Lieutenant Liardet, R.N., and was brought to England in H.M.S. Snake, and presented by that gentleman to P. C. Blackett, Esq., by whose kind permission I was allowed to examine it in detail: the results of this dissection I now beg respectfully to lay before the Society. Owing, however, to the length of time which had elapsed subsequently to its capture, and to the manner of its preservation (in rum),—together with a wound on the inferior part of the neck, and others in the mouth, added to several bruises,—part of my description will not be so perfect as could be desired.

"The bones are very hard, compact, and heavy, having no apertures for the admission of air; but they contain, especially the bones of the extremities, a thin oily marrow. The *foramina* for the trans-

mission of the blood-vessels of the bones are small. The *periosteum* is thick and fibrous.

"The *cranium* is short and broad, and is united into a single bone, with very little appearance of suture or harmony: superiorly it is flattened; posteriorly, towards the *occiput*, it is rounded; it declines obliquely forwards; and when it attains the front of the orbits it is suddenly truncated to meet the superior mandible.

"The orbits are large, and separated only by membrane. Above each orbit there is a *fossa*, which is deeper and broader behind than in front, and which ends suddenly at its union with the orbitar process of the temporal bone. External and inferior to the termination of the transverse ridge of the occipital bone there is a process. The temporal bone has two processes: the tympanic, situated immediately anterior to the last-named process; and the orbitar, situated immediately behind the posterior part of the orbit. The basilar process of the occipital bone is short, ending posteriorly in a single round, prominent condyle, which articulates with the *atlas*. The body of the sphenoid is lengthened, and its pterygoid processes form separate bones. The tympanic bones have the internal process much produced. The *jugum* is very long and thin, attached as usual to the tympanic and superior maxillary bones. The palatine bones are long and thin, meeting posteriorly the pterygoid, and anteriorly the superior maxillary bones.

"The upper jaw is immoveable: the superior mandible long, slender, and a little arched at the point. The apertures for the nostrils are long and narrow. The bones of the superior mandible are of the usual form. The superciliary bones are wanting. The lachrymal bones are small, and fixed to the *cranium*. The turbinated *laminæ* are small, soft, and cartilaginous.

"The lower jaw is long and slender, and composed of three pieces, viz., the body of the bone and its two articulating portions. The coronoid processes are very small. The condyloid process is not elevated above the body of the bone. There is a process produced posteriorly for the attachment of the pterygoid muscles.

"The *os hyoides* has the lateral *cornua* much lengthened, passing upwards posteriorly to the occipital bone, then curved forwards for a short distance upon the temporal bone.

"The vertebral column consists of

Cervical *vertebræ*	13
Dorsal ——	9
Sacral ——	12
Caudal ——	8
In all	42

"The *atlas* is of the usual shape. The *processus dentatus* of the second *vertebra* is flattened laterally; the posterior spinous process short, and the anterior long. The articulating processes are inferiorly produced, as are those of all the cervical *vertebræ*: in the lower of them the processes diverge less than in the upper ones. The posterior spinous process of the third, fourth, fifth, sixth, and

thirteenth *vertebræ* is long: in the remainder this process is short. The transverse processes are short in all except the twelfth and thirteenth *vertebræ*, in which they more nearly correspond with the processes of the dorsal series. The articulation of the bodies of the *vertebræ* is effected as usual. The sixth *vertebra* has the transverse processes extended downwards as much as they may be without the free motion of the neck being impeded: in the seventh, eighth, ninth, tenth, eleventh, and twelfth these processes gradually shorten, and in the twelfth and third can hardly be said to be produced: they lengthen in the fourth and fifth, and in the sixth reach the maximum. In the sixth *vertebra* we notice the commencement of two processes proceeding from the superior part of the anterior face of the *vertebræ*, a little external to the median line, which give firm attachment to the muscles of the neck: in the succeeding *vertebræ* these processes are more fully developed till they reach the tenth, after which we observe no trace of them; but instead of them, in the eleventh, twelfth, and thirteenth we have a very prominent anterior spinous process: in the two last it is bifid. In the last (the thirteenth) the transverse processes are extended laterally, and are curved acutely backwards, leading immediately to the shape of the dorsal *vertebræ*.

"These are nine in number. The first has very extensive motion: in the second the motion is much diminished: and the diminution of motion is continued as far as the seventh *vertebra*, the last two having no motion whatsoever. The posterior spinous processes have less development than is usual in most *Birds*. The anterior ones are very little produced. The transverse processes do not overlap each other. The oblique processes strongly resemble those of the neck. In the first *vertebra* the anterior spinous process is most prominent, and in the second, third, fourth, fifth, and sixth the process is bifid and less prominent.

"The sacral region is composed of twelve bones, all anchylosed together, of which the upper four might almost be regarded as lumbar, for they are unconnected to the *ilia*, except by ligament. The *canalis vertebralis* is broadest in the tenth of these *vertebræ*.

"There are eight caudal *vertebræ*, each furnished with transverse and spinous processes, and also, on their anterior face, with two processes arising one on each side of the median line, measuring in length, on an average, 6 lines. The eighth, or last, is in length 2 inches, conical, with the base towards the body, and having the tip scabrous, for the insertion of muscle: on the superior part of the anterior face there is a groove extending about one third of its length. About half an inch from the tip there is a thickening of substance, giving the appearance of the tip having been originally separate. The *canalis vertebralis* extends a short way down the bone. The seventh *vertebra* is united to the eighth by anchylosis.

"The ribs are nine in number, and of the usual form: the two upper ones are not connected with the *sternum*. The oblique processes are situated halfway between their vertebral and sternal extremities. They commence cartilaginous at the inferior margin of

each rib, and are about 5 lines broad at their origin: towards their termination they spread laterally to the width of 1 inch. As they approach the lower rib they get gradually thinner. In the first and last rib they are totally wanting. The last rib, at its centre, has a surface concave externally, produced by the action of the thigh. The sterno-costal bones are seven in number: the last one curved suddenly at its costal end.

"The body of the *sternum* is long. The keel is much developed at its top, and forms a very acute angle posteriorly, terminated by a small line. The space for the attachment of the middle pectoral muscle is considerably larger than that for the attachment of the great pectoral. On each side of the keel there is a large space, terminating inferiorly in one, owing to the shortness of the middle layer compared with the lateral ones. The keel terminates abruptly inferiorly. The ensiform process has a ridge in the middle, along which and the inferior edge of the keel a membrane was attached (which separated in maceration). The external layers of the bone are, as has been already incidentally noticed, much longer than the middle one: they curve inwards toward each other, and are tipped with cartilage. The sternal *fossa* is large and very distinct. The sternal *apophyses* are very large.

"The coracoid bones are long, strongly formed, and smooth anteriorly; the margin much produced at the superior internal edge, and the ends furnished with long hamuliform processes, extending upwards and downwards. The superior one is attached to the clavicle by the intervention of ligament. The upper part of the *os coracoides* is bent upon itself at an angle greater than a right angle. They are larger at their inferior ends, the inner ends being produced and curved forwards. The glenoid cavity of the bone is situated on the exterior posterior part, and is formed by this bone and the *scapula*, about three fifths of the cavity being formed by the *os coracoides*.

"Each clavicle is turned downwards, and is broader near the coracoid bone, and tapering to the front, where there is a protuberance formed by the junction of the clavicles: this protuberance does not touch the *sternum*. Posteriorly they give off a flat conical process, which goes down internally to the coracoid bone, and is united to the process situated on the posterior part of the *scapula*, immediately inferior to its head.

"The *scapula* is remarkably broad and thin: its neck and head rounded. There are three articulating processes in this bone: one with the *furculum*; another with the coracoid bone; and the third with the *humerus*.

"On comparing the *sternum* and adjacent bones with the *sterna* of some nearly allied *Birds*, we find less development of the keel in the *Loon*, and less development of the lateral wings in the *Auk*, and more in the *Spheniscus*. The differences will be best shown by the following tables:

	Colymbus Glacialis.		*Alca Torda.*		*Spheniscus demersa.*		*Aptenodytes Patachonica.*	
	inch.	lin.	inch.	lin.	inch.	lin.	inch.	lin.
Length of the body of the *sternum*	5	3	4	10	5	10	7	0
Length of the lateral wings. .	3	9	4	0	6	5 n.	8	0
Length of its keel	5	0	5	4	6	5	8	0
Length of the ensiform process	1 n.	0	0	3	0	3	1	2 n.
Length of the sternal *apophysis*	0	3	0	2	0	9 n.	1	0
Half the breadth of the bone at its superior margin . .	1	6	0	10	1	7	2	4
Height of the keel at the superior part	1 n.	0	1	4 n.	1	8	1	9
Projection of the keel, superior to the body of the *sternum*	0	3	0	8	1	3	2	0
Length of the *os coracoides*. .	2	0	1	8	3	3	5	10
Length of the *scapula*......	2	3	2	10	6	5	7	7
Breath of the *scapula* at its neck.................	0	3	0	2	0	7 n.	0	8
Breadth near its inferior angle	0	3 n.	0	2	1	9 n.	2	1 n.

or, in integral parts, the length of the centre of the *sternum* being taken as unity :

	Colymbus.	*Alca.*	*Spheniscus.*	*Aptenodytes.*
Length of the middle of the *sternum*..............	1	1	1	1
Length of the lateral wings. .	$\frac{13}{21}$	$\frac{24}{29}$	$1\frac{1}{10}$ n.	$1\frac{1}{7}$
Length of the keel	$\frac{20}{21}$	$1\frac{3}{29}$	$1\frac{1}{10}$ n.	$1\frac{1}{7}$
Length of the ensiform process	$\frac{4}{21}$ n.	$\frac{1}{23}$ n.	$\frac{3}{58}$ n.	$\frac{2}{12}$ n.
Length of the sternal *apophysis*	$\frac{1}{21}$	$\frac{1}{29}$	$\frac{1}{7}$	$\frac{1}{7}$
Breadth of the superior margin	$\frac{6}{21}$	$\frac{5}{29}$	$\frac{2}{7}$ n.	$\frac{4}{12}$
Height of the keel	$\frac{4}{21}$ n.	$\frac{8}{29}$	$\frac{2}{7}$	$\frac{3}{12}$
Projection of the keel above the body of the bone . .	$\frac{1}{21}$	$\frac{4}{29}$	$\frac{3}{14}$	$\frac{2}{7}$
Length of the *os coracoides*. .	$\frac{8}{21}$	$\frac{10}{29}$	$\frac{4}{7}$ n.	$\frac{10}{12}$
Length of the *scapula*	$\frac{9}{21}$	$\frac{17}{29}$	$1\frac{1}{10}$	$1\frac{1}{12}$ n.
Breadth at its neck	$\frac{1}{21}$ n.	$\frac{1}{29}$	$\frac{1}{10}$ n.	$\frac{1}{12}$ n.
Breadth at its inferior angle. .	$\frac{1}{21}$	$\frac{1}{29}$	$\frac{3}{7}$ n.	$\frac{2}{7}$ n.

" The *humerus* is much flattened. On its posterior aspect there is a large *foramen*, situated under, and occupying the whole of the internal part of its head, which is in form crescentic from before backwards : over the internal and posterior part of it a groove passes. The distal end of the bone has two tubercles for articulation. There are two prominent *trochleæ* on its posterior surface, on which work the two sesamoid bones of the elbow-joint. The form of the larger of these is flattened, and of the smaller trapezoid, with truncated edges.

" The *ulna* is very thin and flat, not quite so long as the *humerus*, rounded slightly at its upper extremity, and still less at its lower one. Its head has a cavity, which receives the posterior tubercle of

the *humerus*. Immediately inferior to this is a prominence on the posterior margin, to which is attached the ligament of the two sesamoid bones. The superior ulno-radial joint admits of little motion, being composed of a convex and plane surface. Near the distal extremity of the bone there are several rough lines for the attachment of muscles. The distal articulating surfaces are three: one with the *radius* anteriorly; another with the first carpal bone inferiorly; and the third with the second carpal bone posteriorly and obliquely downwards.

" The *radius* much resembles the *ulna* in shape. At its head it has two articulations: one superiorly, with the anterior tubercle of the *humerus*; and the other posteriorly, for articulation with the *ulna*. There are likewise two articulations at its distal extremity: posteriorly one for the *ulna*; and inferiorly there is another with the first carpal bone. Near its neck is situated a process for the attachment of muscles. On its superior anterior part a groove runs obliquely, from before backwards, and from above downwards. At the distal extremity there is a similar one, but running in a contrary direction, i. e. from behind forwards.

" The first carpal bone has the form of a trapezium, with three articulating surfaces: a superior one for the *radius*; a posterior one for the *ulna*; and an inferior one for the *metacarpus*. The shape of the second carpal bone is triangular, with articulating processes, and a notch on its inferior edge: one anteriorly for the *ulna*; the other inferiorly for the *metacarpus*.

" The *metacarpus* is composed of a single bone, formed by the union of two. The anterior of the two metacarpal bones supports two *phalanges* of the first finger, and is twice the size of the posterior one, which supports the single *phalanx* of the second finger. The upper end is crescentic, articulated with the first carpal bone anteriorly, and with the second inferiorly. There is a *sulcus* between the ends of the two bones, at their inferior extremity.

" The first *phalanx* of the first finger is a long, broad, and flat bone, tapering gradually from above downwards, united to the *metacarpus* by a flat surface, and connected with the second *phalanx* by a similar articulation. The other *phalanx* is broad and flat, tapering from above downwards. By a similar articulation is attached to the posterior metacarpal bone a *phalanx*, which is flat, long, and tapering from above downwards, superiorly giving off a process which passes upwards for a short distance on the posterior part of the metacarpal bone.

" The bones of the *pelvis* are so much shortened behind that they throw the centre of gravity in a perpendicular line with the *vertebræ*. The length of the *ilia* behind the cotyloid cavity is one third of the length of the body in a *Gull* (*Larus*); one half in the *Loon*; and not quite one fourth of the length of the trunk in the *Patagonian Penguin*. The sacro-sciatic notch is a complete *foramen*. The pubic bones are long and feeble; they are turned forwards and tipped with cartilage. The cotyloid cavity is a perfect *foramen*, with a large process at its postero-inferior part tipped with carti-

lage, and articulated with the *trochanter major*. The thyroid *foramen* is not complete, except by the intervention of a ligament which separates it from the *obturator foramen*. As there is no *iliacus internus*, the superior part of the *os ilium* extends upwards, and lies close to the ribs.

"The *os femoris* is formed as usual, the head being flattened anteriorly, the neck short and thick, the *trochanter major* smooth on its superior posterior surface, and articulated with the process on the *ilium*. Besides the posterior there is also an anterior *linea aspera*. There is a process external to the external condyle, having its inferior surface tipped with cartilage, which acts as a pulley. On its infero-external surface there is a sharp edge. The condyles are not much everted.

"The shape of the *patella* is peculiar. There are two articulating surfaces posteriorly: one which would form part of a large crescent, and which has a prominence for the condyles of the *femur* in its centre; the other, inferior, is likewise crescentic; it is very narrow, and articulated by ligaments to the tubercle of the *tibia*.

"The superior surface of the *femur* has a *crista* in its centre, of an ovoid form: the posterior edge truncated. The internal surface is perfectly flat: the oblique slightly marked with a ridge, and looks downwards. There is a groove on the centre of the anterior edge which also passes obliquely downwards on the external side: these two sides are truncated at their junction.

"The *tibia* is nearly twice the length of the *femur*: the tubercle is elevated above its head, and forms a broad short conical truncated process. On the anterior part of the head there is a large groove, deepest at the top, and passing obliquely downwards and inwards: the outer side is here smooth for articulation with the *fibula*. It has inferiorly two condyles, articulated with the *metatarsus*, having a *foramen* above and between them for the transmission of tendon, &c.

"The *fibula* is in the form of a lengthened cone, and is attached to the outer surface of the *tibia*: for about two thirds of its length it is anchylosed to that bone inferiorly. It has the usual quantity of surfaces for the attachment of muscles.

"There is no *tarsus*.

"The *metatarsus* has two articular depressions on its posterior surface for the reception of the condyles of the *tibia*. It represents three pulleys for articulation with the *phalanges*. On the inner part of the superior face is situated the metatarsal bone of the first toe, connected by ligaments to the large bone. There is a *fossa* on the superior surface, between the first and second, and second and third bones of the *metatarsus*: this gradually decreases in size and increases in depth, till it perforates the bone, and joins the *fossæ* on its inferior surface, where, immediately anterior, internal, and inferiorly to the outer depression on its head, there is a large protuberance forming the inner boundary to a groove. The phalangeal end is formed as in most *Birds*. The first toe, which is the smallest in the foot, has three bones, all of which are flattened, and have

simple articulations, the last one having a nail. The metatarsal bone is only connected to the others by muscle: the whole length of the toe is 1 inch: the second toe has three *phalanges*: the third has four: and there are five belonging to the fourth toe. All are formed as is usual in this class.

"The ligaments of the head and trunk are of the usual form.

"In addition to these a ligament arises from the sesamoid bones of the elbow-joint, which passes to the external or dorsal side of the *carpus*, where it is tied down; it again passes forwards, and is attached by separate slips to the joint and head of the first part of the *metacarpus* and to the first *phalanx* of the first finger; and is inserted into the second about 3 lines from its head.

"The ligaments of the hip-joint are as usual.

"Besides the usual ligaments of the knee-joint there is one which arises together with the crucial, and is attached to the *patella* halfway down the central line. The form of the semilunar cartilages is crescentic, with prolonged horns.

"The ankle-joint has semilunar cartilages of the usual form.

"There are superior and inferior annular ligaments belonging to the *metatarsus*.

"In no other instance is there any deviation from the usual form.

"There is a very large *bursa* situated within the knee-joint.

"The muscles were of a dark red colour, very tough, and having a great deal of cellular membrane amongst them. The *fasciæ* were very thick and strong. In no instance did I observe any tendency to ossification in the tendons. In the tendons of the *perforatus* of the first and second toes there was a sesamoid bone, scarcely equalling in size a mustard-seed.

"The *panniculus carnosus* is very thick and strong, and is divided into three pieces. The first division arises muscular from the lateral parts of the skin of the shoulder, back, and under the wing; from the *fascia* of the muscles of the back; tendinous along the superior edge of the *furculum*; tendinous from the *fascia* covering the muscles of the shoulder; muscular from the blubber over the shoulder-joint; and by a small head from the inferior part of the cervical *fascia*: it passes upwards, uniting anteriorly and posteriorly to its fellow, and is attached, muscular, into the superior transverse ridge of the occipital bone, and to the posterior third of the sides of the lower jaw. The second portion arises from the dorsal *fascia* by five irregular fleshy slips: it passes downwards, and is attached to the blubber covering the back and sides, sending forwards a membranous slip, which is attached to the skin of the *abdomen*. The last portion arises fleshy from the tubercle of the *tibia*, and from the peritoneal *fascia*: and, covering the abdominal muscles, is attached very firmly to the skin of the *abdomen*, sending off two slips, which unite with their fellows over the central line.

"The *occipito-frontalis* is small, arising posteriorly from the *panniculus carnosus*, and inserted anteriorly into the frontal bone, just above its junction with the superior *maxilla*. The *orbicularis palpebrarum* arises from the anterior part of the orbit, immediately an-

terior to the situation of the lachrymal bones, and is inserted into the orbitar process of the temporal bone, from the inferior half of which a muscle arises, passing downwards under the eye, and attached to the inferior part of the optic *foramen*, sending off a slip, which is attached immediately anterior and internal to the orbitar process of the temporal bone. There is most motion in the inferior eyelid.

"Round the entrance of the external *meatus* of the ear there are some muscular fibres observable, but as the part was much bruised, I was unable to separate them: they seem to act as a sphincter.

"The *masseter*, *temporalis*, and *pterygoideus* arise as usual, as does also the zygomatic.

"On the fore part of the neck there are two muscles: one arising from the superior edge of the *furculum*, near its union with the *os coracoides*, and from the recurved portion of the coracoid bone, and inserted into the temporal *fascia*; the other arising tendinous from the superior internal part of the *furculum*, and attached to the outer and posterior part of the tympanic bone.

"The tongue has a *hyoglossus* and *lingualis*, as usual.

"The muscles of the *os hyoides* and lower jaw are as usual.

"There is only one pair of muscles of voice.

"The *recti postici* and *antici*, *obliqui capitis*, *splenii capitis et colli*, *complexi*, *intertransversales*, *interspinales*, *transversalis colli*, *spinales dorsi et colli*, *trapezius*, *cucullaris*, *rhomboideus*, *biventer cervicis*, *trachelo-mastoideus*, *longus colli*, and *scaleni* muscles are large and well defined, arising and attached in the same manner as in most short-necked *Birds*, but especially resembling the muscles of the neck of the *Loon*; as do also the abdominal muscles, and those for the motion of the dorsal *vertebræ*, ribs, and tail.

"The muscles connecting the *scapula* to the trunk resemble those of the *Loon*, but have broader attachments, in proportion as the *scapula* of the *Penguin* is broader than that of the *Bird* referred to.

"The principal differences are in the muscles of the wing and leg.

"The muscles of the wing I shall now describe. The *pectoralis major* arises from the superior part of the *crista* and the external part of the body of the *sternum*, from the *fascia* of the *pectoralis minor*, from the cartilages of the ribs, and from the anterior part of the coracoid bone; over the *crista* it unites with its fellow of the opposite side; it is inserted, muscular, into the anterior superior part of the *humerus*. The *pectoralis minor* arises from the lower part of the *crista* and the interior part of the body of the *sternum*, and from the inferior part of the *furculum* and coracoid bone; its tendon passes over the union of the three bones of the shoulder-joint, moving freely over them, and is inserted, tendinous, into the scabrous surface on the posterior part of the external side of the *humerus*, just below its head. The *coraco-brachialis* arises from the lateral angle of the *sternum* and base of the coracoid bone, and is inserted immediately posterior and a little superior to the *pectoralis minor*. The *subclavius* occupies the usual place, but is

small. A muscle arises from the outer and upper fourth of the membrane between the *furculum* and *os coracoides*; it passes upwards, but internal to the capsular ligament of the joint; and is inserted, tendinous, immediately above the insertion of the *pectoralis minor*. Another muscle arises from the external inferior third of the *os coracoides*, from the angle and costal part of the *sternum*, and from the *fascia* of the *pectoralis major* for about the length of an inch; passing upwards it forms a round tendon about $\frac{3}{4}$ of an inch from the shoulder, which passes over the joint and under the *supra-spinatus*, and is inserted into the external edge of the *foramen* at the head of the *humerus*. The *supra-spinatus* is small, and arises fleshy from the superior edge of the *scapula*, near the glenoid cavity; it passes round and constricts the ligament of the joint, and is inserted, tendinous, into the *humerus*, immediately anterior to the muscle last named.

"I will here notice, before proceeding to the remaining muscles, a loop through which several of the muscles pass. It arises flat from the infero-anterior edge of the *scapula*, just below the glenoid cavity, and passing upwards and outwards for about an inch, is then doubled upon itself, and attached to the same part from whence it arose: there is no admixture of its fibres.

"A muscle arises from the *fascia* which covers the last rib and the outer edge of the external oblique, passes upwards and through the loop, and is inserted into the lower part of the external edge of the *foramen* situated at the posterior part of the head of the *humerus*. The *latissimus dorsi* arises from the last cervical and first five dorsal *vertebræ*, and forms a tendon, which passes through the loop and is inserted immediately below the preceding muscle. The *infra-spinatus* arises fleshy from the whole external surface of the *scapula* below the upper third, and is inserted into the large tubercle of the *humerus*. A muscle arises from that part of the inner edge of the *os coracoides* which is produced; it passes obliquely upwards and outwards behind the *os coracoides*, to which it is attached; and is inserted tendinous into the anterior tubercle of the *humerus*. The *deltoides* arises from the posterior part of the projecting edge of the *scapula*, and from the scapular process of the clavicle; passing over the shoulder-joint, it is inserted into the anterior part of the middle tubercle of the *humerus*. The *subscapularis* arises from the internal surface of the *scapula*; it passes upwards, and is inserted into the posterior part of the middle tubercle of the *humerus*. The *teres minor* arises from the whole width of the posterior surface between the glenoid cavity and the end of the upper third of the *scapula*; it passes in the groove, and is inserted into the inferior part of the large tubercle of the *humerus*. Of the *triceps extensor cubiti* the long head arises immediately above the origin of the *teres minor*, and passing down on the external side of the *humerus*, it is joined by the second head, arising from the internal part of the large *foramen cæcum* of the *humerus*; these two unite about the middle of the arm, and are joined by the third head, which arises from the two inferior thirds of the posterior edge of the *humerus* till within 8 lines of the joint: it

is now attached to the sesamoid bones of the elbow-joint, and to the *fossa* on the inferior parts of the posterior surface of the *os humeri.*

"The *anconeus* arises from this muscle, and from the part of the bone below the origin of the third head, and is attached to the sesamoid bones anterior to the *triceps extensor cubiti.* Instead of a *biceps* and *brachialis internus,* there is a *triceps flexor cubiti,* the long head of which arises, tendinous, from the antero-interior part of the superior angle of the *furculum,* and, passing over the joint, is joined, at the union of the upper with the middle third of the *humerus,* by the fibres of the middle head, which arises fleshy from the *furculum* immediately behind the *foramen* formed by the union of the three bones of the shoulder passing on to join the long head; at the head of the *humerus* it is joined by the short head which arises from the anterior part of the *foramen cæcum*; when it reaches the superior part of the middle third of the *humerus,* it joins the other tendons, and then forms an *aponeurosis* over the elbow-joint, and is attached to the middle part of the *radius.* A muscle arises from the anterior superior edge immediately below the arterial groove on the lower part of the *humerus*; it passes directly downwards and is inserted into the radial extremity of the metacarpal bone and into the edge of the carpal ligament. The *flexor communis* arises from the internal side of the *humerus,* from the ligament of the elbow-joint, and from the superior part of the *radius* and *ulna*; it divides into two tendons, which go down in the interosseal space, passing under the *ligamentum carpi annulare posterius,* and are attached to the first and each succeeding *phalanx* of the two fingers about 5 lines below their articulations. The *extensor communis* has the same situation and number of attachments on the external or dorsal side of the *humerus.* There is a *pronator quadratus* arising as is usual in this class. There is also a muscle which arises from the anterior part of the *radius* at its distal extremity, and is inserted into the projection of bone formed by the *phalanx* of the second finger, and also, by a slip, into the internal part of the first *phalanx* of the first digit.

"The muscles serving for the motion of the inferior extremity may be described as follows.

"The *rectus* arises by a *fascia* from the spinous processes of the last three dorsal and two lumbar *vertebræ,* and muscular from the lower half of the external part of the *dorsum ilii* and sacro-iliac *symphysis*; and, passing over the neck of the thigh-bone, is inserted into the lower edge of the groove on the anterior part of the *patella.* The *tensor vaginæ femoris* arises by a *fascia* from the sacral *vertebræ,* passes over the cotyloid cavity and *trochanter major,* and turning to the anterior part of the thigh is joined by another head which arises immediately anterior to the cotyloid cavity; after this union they are inserted into the *fascia* of the thigh about halfway down. The *glutæus medius* at its origin occupies that part of the *dorsum* which extends between the origin of the *acetabulum* and the ridge situated in the centre, and passes downwards and is inserted into the *trochanter minor* and the ridge which joins it. The *glutæus minimus* arises from

the whole of the *dorsum ilii* unoccupied by the other *glutæi* except its *crista*, and is inserted into the anterior part of the *trochanter major*. The *glutæus maximus* arises from the prominent ridge on the *os ilium* below the *acetabulum*; it passes on the posterior surface of the thigh-bone; and when it has passed below the head of the *tibia* it forms a round tendon and passes through a loop situated on the external posterior part of the *tibia*; continuing its course obliquely downwards, it is inserted into the scabrous ridge on the posterior surface of the *tibia* near its head. A muscle arises from the transverse processes of all the caudal *vertebræ* except the last, goes forwards, and is attached to the postero-internal edge of the *tibia* just below its head. Another muscle arises from the anterior part of the last caudal *vertebræ*, and is inserted into the external part of the *linea aspera* after its bifurcation. The *pyriformis* arises from the anterior oblique processes of the caudal *vertebræ*, from the tip of the *ischium*, and from the internal part of the *os pubis*; the fibres converge downwards, and are inserted into the intero-anterior ridge of the *tibia* just below the tubercle. The *semitendinosus* arises from the ridge immediately anterior to the *glutæus maximus*, and is inserted immediately inferior to the bifurcation of the *linea aspera* on its external division. The *gemini* arise from the *ischium* immediately posterior to its spine, and are inserted into the cavity posterior to the *trochanter major*. A muscle arises from the *ischium* anterior to the *gemini*, and is inserted into the intero-anterior ridge of the *tibia*, just below the *pyriformis*. Of the *triceps adductor femoris* the first head arises from the extero-inferior part of the *pubis*; the second head arises immediately above the first; and the third above the second, and from the interosseous ligament which unites the *pubis* and *ischium*: they join on the upper third of the thigh, and are attached to the *linea aspera* on its internal side and division. The *obturator internus* arises fleshy from the internal part of the *pubis*, from part of the *obturator foramen*, and from the *ischium*; it forms a tendon which passes through the thyroid *foramen*, is tied down to the joint, and is inserted into the anterior part of the great *trochanter*. A muscle arises from the outer edge of the cotyloid cavity, passing outwards and a little upwards, and is inserted behind the *trochanter major*. Another muscle arises from the anterior part of the *acetabulum*, passing directly outwards, and is strongly attached to the ligament of the joint; it is inserted into the thigh-bone just below its neck.

" A muscle arises from the interior and a small part of the anterior and posterior surfaces of the thigh-bone, from near its neck to the condyles, and forms a tendon which is inserted into the ridge at the anterior internal part of the *tibia* immediately below its head. The *cruralis* arises fleshy from all the superior and external parts of the bone not occupied by the former; one part is inserted into the whole of the superior surface of the *patella*, the remainder passes over the internal part of the *patella* and is attached to the internal side of the head of the *tibia*. A muscle arises by four heads: the first, tendinous, from the ridge behind the external condyle which formed the loop through which the *glutæus maximus* passed; the second, fleshy,

from the internal side of the *triceps*; the third, from the inferior portion of the intero-anterior ridge of the *tibia*; the fourth, from the inferior internal edge of the *patella*; these two last join just below the origin of the third, and passing down tendinous are united to the two other tendons a little above the ancle-joint: it expands and flattens at the joint, and just below it divides into two tendons, the internal of which is inserted into the internal edge of the groove on the plantar surface of the metatarsal bone, while the external tendon is inserted into the external head of the same bone. Another muscle arises from the postero-inferior part of the cotyloid cavity, passes forwards on the exterior part of the thigh and over the groove on the *patella*, and is attached on the interior part of the head of the *tibia*. The tendon of the *flexor perforatus* is composed of four muscles, which unite just above the ancle-joint. The first arises by two heads, one from the outer surface of the external, and the other from the inner side of the internal condyle; about the end of the upper third of the *tibia* this forms a tendon, which passes down to the place of junction with the others: the second has also two heads, one from the posterior part of the head of the *fibula*, and the other immediately below the attachment of the *glutæus maximus*; the muscle forms its tendon just below the middle of the bone, and passes forwards and joins that of the first muscle: the third has one origin between the two condyles, and forms its tendon at the middle of the leg, passing on and joining the two former: the fourth muscle arises immediately above the third, and forms its tendon like the rest, joining them above the ancle: after the tendons are united they are distributed as usual. The *flexor perforans* consists of two heads; the first arises from the back part of both condyles; the second arises from the superior and posterior third of the *tibia*, *fibula*, and interosseous ligament: they unite about halfway down the bone and form a tendon, which passes in the groove of the plantar surface of the metatarsal bone, and is distributed in the usual manner. A muscle arises from the scabrous surface situated on the internal part of the posterior face of the *tibia* about halfway down that bone, and forms a tendon which is attached to the upper part of the internal edge of the groove in which runs the tendon of the *perforans*. Another muscle arises from the external condyle, from the *patella* on its anterior surface, and from the fibres of the *rectus femoris*; it covers the *tibia* and fills up the space between it and the *fibula*, and forms a tendon which passes through the *foramen* situated at the anterior surface of the *tibia* between its condyles, under the capsular ligament of the ancle-joint, and is attached to the prominence situated between the second and third portions of the metatarsal bone near its tibial extremity.

"A muscle arises from the anterior and external parts of the head of the *fibula*; it becomes tendinous about halfway down the leg, passes under the annular ligament, and is inserted into the external side of the metatarsal bone near its postero-inferior angle: another slip goes under the foot and forms the plantar *fascia*. Another muscle arises from the anterior inferior surface of the *patella*, and from the whole of the *fossa* and its edges on the head of the *tibia*,

passes downwards, and is tied down by the annular ligament; and has the same distribution as in the *Loon* and *Gull*, except that the tendon is more closely tied down, smaller, and not so round. Another muscle arises fleshy from the whole anterior part of the *fibula*, interosseous ligament, and part of the external side of the *tibia*; it forms its tendon near the ancle-joint, and is attached to the postero-external angle of the *metatarsus* on its plantar surface. There are also four muscles arising from the metatarsal bone, one on each side, and one in the *fossæ* between the three portions of the metatarsal bone: they all arise near the tibial end on its superior surface, and are attached to the *phalanges* of the first, second, and fourth fingers. The thumb has three muscles: an *extensor*, on its superior surface; a *flexor*, on its inferior; and an *abductor*, on its internal surface; all attached to the tibial end of the *metatarsus* as usual.

"The diaphragm consists of twelve narrow fleshy slips, which arise, six on each side, from the internal surface of the ribs: near their angle they pass upwards, and are inserted tendinous into the thin transparent membrane covering the lungs. The blood-vessels pass in front of it.

"The circulatory system corresponds exactly with that of the *Loon*, except in the origin and distribution of the arteries of the stomach. The cœliac artery comes off on a level with the fifth rib; it passes a little forwards, and divides into the *coronaria ventriculi*, the hepatic, and the splenic. The *coronaria ventriculi*, just after its origin, divides into the superior and inferior coronaries: the superior passes round the large curvature of the stomach, and near the *pylorus* gives off the superior pyloric and left hepatic; the inferior passes down the right side of the stomach, and disappears at the *pylorus*, being here minutely ramified upon it. The hepatic gives off the right gastro-epiploic, which goes on the inferior angle of the stomach, and the right gastric, which goes on the *pylorus* and superior part of the stomach, anastomosing with the superior pyloric and inferior coronary arteries. The splenic gives off a small artery distributed on the cardiac portion of the stomach, and some *vasa brevia*, which are distributed to the left portion of the stomach.

"Not wishing to mutilate the skeleton, I did not examine the brain; but from the number, size, and situation of the *foramina* in the base, and the whole contour of the *cranium*, the brain must be presumed to be very nearly similar in proportional quantity and structure to those of the *Loon* and *Gull*.

"The nerves are distributed as usual. The brachial plexus is composed of the last cervical and first two dorsal nerves, and of a filament from the last spinal nerve but one in the cervical region. The sciatic is composed of the five superior or anterior pairs of pelvic nerves.

"The nose is organized similarly as in others of this class. The cartilaginous *laminæ* of the turbinated bone are concentric, and thirteen in number.

"The eye has six muscles, which arise and are attached as usual. The lachrymal gland is placed at the postero-superior part of the

orbit, and is large in proportion to the globe of the eye. It sends off several ducts; I think seven; but the part being much injured, I found it impossible to ascertain their precise number and origin: one, however, opened immediately under the anterior part of the *membrana nictitans.* Two other ducts also opened below this membrane, passing from the Harderian gland, which was situated at the inferior part of the orbit. The nasal gland occupied its usual situation, partly in the anterior and superior portion of the orbit, and partly in the *fossa* of the frontal bone: its duct passed forwards under the bridge of bone, and then bifurcated, one division of it ending on the cartilaginous *laminæ* of the *ossa turbinata*, and the other going forwards, and lying on the bone: I was not able to trace it further.

"The *membrana nictitans* is large and strong: it is moved by a *pyramidalis* and a *quadratus* muscle.

"The globe of the eye is large, as compared with the *cranium.* The sclerotic is less osseous than I have yet found it in any *Bird.* The optic nerve enters at the postero-inferior part of the sclerotic. The *cornea* is small, owing to the large space occupied by the sclerotic. Under the *cornea* lies the *membrana aquatica*, consisting of a thin membrane, adhering to the edge of the *iris.* This membrane was first observed, together with the *tunica cellularis*, by Mr. Blackett, in 1802, in the eye of the *Cat*, the preparation of which was sold in the first part of Mr. Brookes's Museum. The *tunica cellularis* in this animal is rather pulpy, but, on the application of *liquor potassæ*, it dissolved, and displayed a cellular structure. Mr. Blackett demonstrated this membrane to me in 1832, since which time I have observed it in all the eyes I have examined; but, owing to the difficulty in obtaining specimens, I have not been able to make sufficiently extensive researches to justify the demonstration of the membrane as one of the proper tunics of the eye. There appears to be a *marsupium nigrum.* The *retina* is very thick and strong.

"The absorbent system is more perfect than in most *Birds.* Of the thoracic ducts, the left is the largest. There are a femoral and two axillary glands; also an extra pair of bronchial glands more than in the *Loon* or *Gull.* The coccygeal glands are 2 inches 3 lines long, and 9 lines broad.

"There is a gular pouch, which measures in length 4 inches, and in breadth 8 lines.

"The tongue is set with cartilaginous *papillæ* directed backwards.

"There is only one pair of salivary glands; the submaxillary.

"The structure and proportion of the lungs are the same as in the *Water Birds* generally. The air-cells are few in number, and small, and are filled by openings from the lungs, or from one cell to another. They consist principally of the internal air-cells; one above the *furculum*; and the axillary, abdominal, and femoral rows.

"The liver, spleen, and *pancreas* are large.

"The *œsophagus* is straight, and 1 inch and 5 lines in width. It is infundibuliform, so that when it reaches the stomach it is 2 inches and 4 lines wide: the *infundibulum* contained the beaks of cuttlefishes and gravel.

"The stomach is muscular, small, and glandular, and of the shape of an egg. The *duodenum* is broad at its origin, and at about 3½ inches from its commencement the biliary and pancreatic ducts enter. The gall-bladder is 6 inches long and 2 inches in circumference; it is attached to the under side of the liver, and, gradually diminishing in diameter, it passes over the stomach, and is inserted into the intestine, without the intervention of any duct.

"The *testes* were large, as were the supra-renal glands and kidneys. I did not observe any difference from the usual structure and proportions in any other parts.

"The small intestines measured 22 feet 6 inches in length, and were about the thickness of the little finger. There were attached to them two *cæca*, each measuring about 1 inch 3 lines in length, which were of the same diameter as the intestines. The great intestines were somewhat larger than the small. The measurements of the stomach and the intestines were as follows:

	Feet.	Inches.	Lines.
Length of the *œsophagus*	0	10	0
Breadth at the *pharynx*	0	1	6
——— *infundibulum*	0	2	4
Length of the *infundibulum*	0	10	0
Breadth at the junction of the *infundibulum* with the stomach	0	6	0
Length of the stomach	0	4	0
Width of ditto	0	2	6
Length of the *duodenum*	1	3	0
Circumference of ditto	0	4	0
Length of the small intestines, inclusive of the *duodenum*	22	6	0
Length of the *cæca*	0	1	3
Circumference of the *cæca* and the small intestines	0	2	6
Length of the large intestines	0	6	0
Circumference of ditto	0	2	9

"The total length of the individual examined, measured over the back, was 3 feet 2 inches and 6 lines; the length of the neck, 11 inches and 9 lines; that of the trunk, 1 foot 1 inch and 9 lines."

The reading of Mr. Reid's communication was illustrated by the exhibition of the skeleton of the specimen of the *Patagonian Penguin* described by him, and of preparations of many of the *viscera*, the whole forming part of the collection of Mr. Blackett.

LXVI. *Intelligence and Miscellaneous Articles.*

ON THE EVOLUTION OF LIGHT DURING CRYSTALLIZATION. BY HENRY ROSE.*

AN emission of light has often been noticed during crystallization, but its appearance has always been a casual one, and never, as far as I am aware of, has it been produced at will. I have observed during

* Read to the Academy of Sciences at Berlin, July 30, 1835.

the crystallization of arsenious acid a strong emission of light, which differs from that seen during the crystallization of other substances, in as much as it may be produced at pleasure. Take two or three drachms of the transparent or vitreous arsenious acid, put it in a matrass of white glass along with an ounce and a half of not fuming muriatic acid of the common strength, and half an ounce of water; allow the whole to boil for ten minutes or a quarter of an hour, and then let it cool as slowly as possible, which is best done by gradually decreasing the flame of the spirit-lamp which has been used for the boiling. If the experiment is conducted in a dark room, the crystallization is accompanied by a strong emission of light, the formation of each little crystal being attended by a spark. If the vessel is then agitated, a great number of crystals suddenly shoot up, and an equal number of sparks occur at the same time. If a considerable quantity of arsenious acid, such as an ounce or an ounce and a half, or more, is treated with a corresponding quantity of diluted muriatic acid, then, on shaking the vessel, if the right moment is seized, the emission of light from the shooting of the crystals is so powerful that a dark room may be lighted up by it.

Considerable time elapses before the acid solution of arsenious acid leaves off depositing crystals, consequently the cooled solution still continues to emit light on the second and even on the third evening, but only extremely feebly, and only when it is agitated. It is, however, impossible after this to produce any emission of light; a proof that it is occasioned by the shooting of the crystals, and not by electricity of friction.

If the hot solution of the transparent arsenious acid is allowed to cool rapidly, whereby a friable mass of arsenious acid is obtained, then either a very feeble light or none at all can be observed. Equally little light is observable if the transparent acid is treated with acetic or nitric acid, the latter either of the common strength or fuming. The reason of this is simply that these acids dissolve but very little of the arsenious acid, especially the acetic acid, so that this solution is but slightly tinged yellow by sulphuretted hydrogen, without any sulphuret of arsenic being precipitated. Dilute sulphuric acid, on the other hand, dissolves rather more arsenious acid by boiling, and if this solution be allowed to cool very slowly, a feeble light may sometimes be observed. If a large quantity of the transparent arsenious acid is treated with only so much nitro-muriatic acid (which, however, must contain an excess of muriatic acid,) that it is not completely dissolved and oxidized to arsenic acid, a strong light is then observed on cooling.

The cause of the luminosity of crystals during their formation has long appeared to me to be this: that the substance which separates from a fluid in the form of a luminous crystal is not contained as such in the solution, but that it is only formed when the crystal is formed, and that the appearance of light is necessarily conditioned by the formation of a new substance in a crystalline state.

The light evolved during the crystallization of substances has most frequently been observed with sulphate of potash, but always only

casually, and never during the recrystallization of pure sulphate of potash, but, as I believe, merely during the crystallization of the solution of the residue from the preparation of nitric acid. This contains almost always sesquisulphate of potash, which as such is soluble in water, but which, according to Phillips, is decomposed whilst crystallizing into bisulphate and neutral sulphate of potash; and the latter becomes luminous during crystallization, whilst it is formed in the fluid, and crystallizing out of it.

Two isomeric states of the arsenious acid are commonly known: it is either transparent and vitreous, or porcellanous and opake. At first after melting it is quite transparent, but simply by keeping it, and without its experiencing any increase of weight, it becomes milk-white and opake. In both states the acid has different specific gravity and solubility in water.

I have only been able to observe the evolution of strong light during the crystallization of the arsenious acid, when I treated the vitreous acid with muriatic acid in the above-mentioned manner. In the same manner the opake acid and also the pulverulent arsenious acid, which is obtained by sublimation during the roasting of the arsenical ore, and which is known in commerce under the name of "Giftmehl*," when treated with muriatic acid did not produce, even by the most gradual cooling, any light, and it was only by shaking the vessel that a very feeble light was visible; in the latter case most likely because the opake acid contained still some portions of the vitreous acid. But this feeble light could never be compared with the strong light which was visible when the transparent acid was employed. The light evolved during the shooting of the crystals of the arsenious acid appears, therefore, to depend upon this, that the solution of the transparent acid is changed by crystallizing into the opake or porcellaneous kind. The crystals produced belong, therefore, to the opake modification; and the change of the transparent into the opake acid is caused by nothing else than the transformation of the acid from a completely uncrystalline to a crystalline state.

The crystals of arsenious acid which are obtained from a very slowly cooled solution in muriatic acid are, however, transparent; but this transparency is caused only by their size, and an aggregate of very small crystals of the acid would exhibit an opake appearance. The crystals formed were always regular octohedrons, and did not possess the form observed by Wöhler, which is, perhaps, a third isomeric modification of arsenious acid.

If the transparent acid is treated in the above-mentioned manner and proportions, and the crystals have been formed accompanied by phosphorescence, and the whole been allowed to cool perfectly, the phosphorescence can be obtained once more, and sometimes even very powerfully, if the whole is again heated to the boiling-point and slowly cooled. However, the light is much more feeble than that first observed, and is only caused by the muriatic solution still containing portions of the transparent arsenious acid, and which during crystal-

* The suboxide of arsenic of Berzelius.

lization evolves this feeble light. Moreover, the quantity of dilute muriatic acid in the mixture above-described is not sufficient to dissolve all the arsenious acid, and there remains, therefore, a small portion in the vitreous state.

But still all the appearances of light which have been observed cannot be explained on the principle of a new arrangement or formation, and I myself hold this hypothesis to be one which requires the evidence of more facts to establish its probability. Thus, Berzelius observed phosphorescence during the crystallization of fluoride of sodium out of a solution which held the same salt already in solution.

HYDRATE OF OIL OF TURPENTINE.

MM. Dumas and Peligot state that they received from M. Julien Fontenelle some well-defined crystals collected from oil of turpentime: 0·287 of these, submitted to analysis, yielded 0·295 of water and 0·662 of carbonic acid, which indicate:

Carbon	63·8	
Hydrogen	11·4	
Oxygen	24·8	100·0

M. Bonastre had observed prismatic crystals resembling the preceding in the *essence de basilic* (*Ocymum basilicum*); of these 0·285 yielded 0·297 of water and 0·657 of carbonic acid, or

Carbon	63·8	
Hydrogen	11·5	
Oxygen	24·7	100·0

Lastly, on examining the oils in the laboratory at the Jardin des Plantes, a bottle of oil of the *Cardamomum minus* was found in which there were numerous colourless prismatic crystals like the foregoing. They were purified by pressure, and 0·213 of them gave 0·493 of carbonic acid and 0·220 of water, or

Carbon	64·	
Hydrogen	11·4	
Oxygen	24·6	100·0

These numbers nearly coincide with those which give the following formula:

C^{40}	1530·4	63·6
H^{44}	275·0	11·4
O^{6}	600·0	25·0
	2405·4	100·0

This formula may be explained by $C^{40}\ H^{32} + H^{12}\ O^{6}$.

If, as the authors believe, these three substances are identical, the hydrate which constitutes them ought to be found in a multitude of cases. If they are merely isomeric, the study of them would require more of them than the authors possessed; their object in publishing these analyses is to call the attention of chemists to a product which is but little known, but of considerable interest.—*Ann. de Chim. et de Phys.*, tome lvii. p. 334.

ACTION OF OXACIDS ON PYROXYLIC SPIRIT.

[Continued from page 429.]

The oxacids, by their action in pyroxylic spirit, give rise to two sorts of products: true neutral salts, corresponding to compound æthers formed from alcohol; and true acid salts, corresponding to sulphovinic or phosphovinic acid. The former are very easily obtained, and all contain an atom of acid, an atom of méthylène, and one of water. They are more easily prepared, more stable and volatile than the corresponding alcoholic compounds. The detailed examination of one of these compounds will show the theory of their formation; the neutral sulphate of méthylène, selected for this purpose, has no corresponding compound in the alcohol series, at least not so pure.

Sulphate of méthylène.—Distil a mixture of one part of pyroxylic spirit, with eight or ten parts of concentrated sulphuric acid. As soon as ebullition commences, an oleaginous liquor distils, and when it is over, this is found to be equal in quantity to the spirit employed; the distillation ought to be slow, but the boiling ought to be kept up.

With the oleaginous liquid there distils an aqueous one; the mixture is to be agitated with a little water, and then with chloride of calcium. It is then to be rectified from finely powdered caustic barytes. Lastly, it is to be exposed to sulphuric acid and potash *in vacuo*.

These operations are to separate the sulphuric and sulphurous acids and water; the only product which remains is colourless, has an alliaceous smell, and its density is 1·324. It boils at 360 Fahr. without suffering any alteration. The density of its vapour is about 4·565.

It is composed of

1 atom of sulphuric acid	501·16	64·5	
1 atom of méthylène	178·05	22·4	
1 atom of water	112·50	13·1	——100·0

This neutral sulphate of méthylène may not only be distilled without alteration, but it does not decompose at 392° Fahr. It is slowly decomposed by cold water, and very rapidly by boiling water, which immediately destroys it with violent disengagement of heat; the sulphate totally disappears without producing any new oil; sulphométhylic acid is formed, and hydrate of méthylène (pyroxylic spirit) is reproduced.

This compound has no action upon caustic barytes; on the contrary, hydrate of barytes and the hydrated alkalies in general decompose it with the greatest facility: when, for example, a solution of potash is mixed with it, much heat is excited; the sulphate of méthylène disappears, and sulphomethylate of potash and pyroxylic spirit are produced in its place. In this reaction the water is found to reproduce pyroxylic spirit.

This decided action of the dissolved alkalies, as well as analysis, prove a difference between the neutral sulphate of méthylène and the neutral sulphate of carbonated hydrogen of Sérullas, which would create surprise if it were not noticed: the former contains one atom

of water, the latter only half an atom, and treated with potash it yields an oily carburetted hydrogen, while the sulphate of méthylène yields none. It is supposed, however, that the compound obtained by Sérullas is, in fact, a sulphate of méthylène, mixed with variable quantities of oily carburetted hydrogen.

Sulphate of méthylène possesses very important properties, since by its use all the analogous compounds of méthylène may be procured.

When heated with fused common salt, sulphate of soda and gaseous hydrochlorate of méthylène are produced.

Heated with fluoride of potassium, gaseous hydrofluate of méthylène is obtained, hereafter to be described.

When it is heated with cyanuret of mercury or cyanuret of potassium, sulphate of mercury or of potassium is procured, and at the same time, hydrocyanate of méthylène is obtained in a liquid form. When distilled with benzoate of potash, benzoate of méthylène is procured, and with dry formiate of soda, formiate of méthylène results. When put in contact with saturated alkaline sulphurets, a liquid is obtained analogous to the *mercaptan* of M. Zeise.

[To be continued.]

PREPARATION OF PURE TELLURIUM.—BERZELIUS.

Telluret of silver having lately been found in Siberia, and telluret of bismuth at Schemnitz, Berzelius has obtained the metal in a pure state from the former by the following process: Mix dry carbonate of potash intimately with the well-pulverized mineral, make it into a thick paste with olive oil, and put it into a porcelain crucible with a cover. The crucible is then to be at first gently heated, till the oil is carbonized; and when gas ceases to burn at the edges of the crucible, the heat is to be raised for a moment to whiteness, and the crucible then allowed to cool. A deep brown porous mass is obtained; it is to be quickly powdered in a dry mortar, and thrown upon a dry filter and washed with boiling distilled water, with as little contact of air as possible.

A liquor of a rich red colour is obtained, which whenever it comes into contact with the air, becomes of the lustre of silver from the tellurium which separates, while the potassium oxidizes by the oxygen of the air. As soon as the liquor passes colourless, the mass on the filter is sufficiently washed, and is composed of charcoal and metallic bismuth, containing mere traces of tellurium.

The deep red solution contains telluret of potassium mixed with more or less sulphuret and seleniuret of potassium, with a small quantity of telluret of gold, copper, manganese, and iron. If the solution be suffered to remain at rest, the surface becomes covered with a pellicle of tellurium, and gradually, but very slowly, it becomes turbid to the bottom: by blowing air into it the mass oxidizes readily. The potassium becomes potash, and the tellurium is precipitated in the metallic state; it may be said that the tellurium is precipitated by oxygen. When the precipitation has ceased, the solution assumes a green colour, and if it be poured off at this moment, it deposits in a few seconds a very small quantity of tellurium, and the liquor becomes

yellow when the precipitation has ceased. The green colour is owing to a mixture of the blue tint occasioned by the small quantity of tellurium mixing with the yellow colour of the liquor. Sometimes the remaining liquor is of a dull rose colour, and gives no precipitate in several days; this is owing to the telluret of iron which it contains.

As long as the potash is in access, the sulphur and the selenium are not precipitated, but the access of air converts them into acids; this is a method of obtaining tellurium free from these substances. Muriatic acid precipitates from the yellow solution the selenium and the sulphuret of tellurium which it contains, in the state of sulphuret and seleniuret of tellurium.

The tellurium precipitated from the alkaline solution is a very fine and dense powder: it must be purified by distillation; but on account of its slight volatility it cannot be sublimed from a retort in a common furnace. In order to effect it, a long porcelain vessel, containing tellurium, was put into a large porcelain tube in a furnace; it was heated to redness, and a current of hydrogen gas passed over it. The tellurium was converted into vapour, and it was constantly carried by the hydrogen towards the cold parts of the tube, where it was condensed. In order to make the tellurium flow after its condensation, the tube must be slightly inclined. In a short time all the tellurium distils, and there remains in the porcelain vessel a small button formed of the tellurets of gold, copper, and iron; the product of the distillation is pure tellurium.

In general the process, which consists in fusion with potash and charcoal, may be employed to purify tellurium, especially if it contains sulphur, selenium, or arsenic, all bodies which cannot be separated from it by distillation. The arsenic goes off in vapour at a red heat, and the two others, after the precipitation of the tellurium by the air, remain dissolved in the liquor. The solution of potash contains the metals which render the tellurium impure. If in this operation powdered charcoal be employed instead of oil, the mixture may be strongly heated at once, but the solution of telluret of potassium which is then obtained contains telluret of calcium; and as the lime which is produced is precipitated with the metal, the precipitate must be first washed with muriatic acid, and then with water. The quantity of charcoal ought always to be sufficient to prevent the mass from fusing during reduction, for then it would go over the edges of the crucible and part of it would be lost.—*Annales de Chimie et de Physique,* lviii. 132.

ANALYSIS OF THE PLENAKITE. BY PROFESSOR BISCHOFF.

This very rare mineral is found in the shallow part of an iron mine at Framont (Vosges); it is crystallized in rhomboids. Its physical characters are the same as those of the plénakite of Nordenskiold, and the analysis of M. Bischoff confirms their similarity. It is as follows:

Silica	17·048	
Glucina	14·280	
Lime and magnesia...............	0·030	
Matter unacted upon	2·252	—33·610

It is considered as a bisilicate of glucina.—*Ibid.**

* See p. 239 of the present number.

MINERAL WATERS.

Circular addressed by Professor Daubeny of Oxford to men of science, particularly on the Continent, who interest themselves on the subject of mineral waters:

"August 25, 1835.

"Sir,—The British Association for the Advancement of Science, at their meeting in Dublin this year, having done me the honour to request that I would draw up a report "On the present state of our knowledge with regard to Mineral Waters," I am anxious to obtain all the information I can on this subject, and shall be obliged by any communications with which you may favour me, especially relating to such recently published memoirs or treatises as consider the subject in a scientific point of view.

"These communications, if from foreign parts, may be addressed to the care of Mr. Hunnemann, Queen-street, Soho-square, London, who has correspondents in most cities of Germany.

"I am, Sir, your obedient servant,

"Charles Daubeny, F.R.S.,

"Professor of Chemistry, Oxford."

NEW WORK PUBLISHED BY DR. BOUÉ.

Guide du Géologique Voyageur. 2 vols. small 8vo. Levrault, Paris.

This work is upon the model of Leonhard's 'Agenda Geologica,' but much enlarged.

It is divided into seven parts:

1. Preliminary remarks, viz. indications for the travelling geologist; necessary instruments of all kinds; maps, books, &c.
2. Considerations of the external configuration of the earth's surface; physical geography; mountain chains; valleys; rivers; springs, &c.
3. General geology; description of rocks, and changes produced in them by fire; table of formations on which the crystalline schistose rocks are placed as modifications of various older and newer formations.
4. Particular geognosy; primary, secondary, tertiary, and alluvial rocks; their various epochs.
5. Palæontology; distribution, &c. of fossil plants and animals.
6. Geological geography; general review of the geology of known countries; geological itinerary in the best-known countries of Europe.
7. Applications of geology.

The Appendix contains: 1st, catalogue of the best geological maps; 2nd, catalogue of the best works of geological descriptions; 3rd, catalogue of all known collections of rocks and fossils. Under each article there are numerous references to the best works or memoirs on each particular branch of geological research.

Sir Francis Chantrey's admirable bust of the late venerable Troughton (now placed in the Observatory at Greenwich) has, by permission, been modelled with great success as a cabinet bust by Mr. C. A. Rivers, of 26, Derby-road, Kingsland, the artist by whom those of Telford, Lord Brougham, Lord Denman, &c. were executed.

Meteorological Observations made by Mr. THOMPSON *at the Garden of the Horticultural Society at Chiswick, near London, and by Mr.* VEALL *at Boston.*

Days of Month. 1835.	Barometer.			Thermometer.			Wind.		Rain.	
	London.		Boston.	London.		Bost.	Lond.	Bost.	Lond.	Bost.
	Max.	Min.	$8\frac{1}{2}$ A.M.	Max.	Min.	$8\frac{1}{2}$ A.M.				
Oct. 1	29·305	29·220	28·63	59	50	56	s.	w.	0·53	0·06
2	29·357	29·182	28·70	60	40	57	s.	s.	·23	·09
3	29·379	29·207	28·88	58	40	49	se.	calm	·21	·21
4	29·451	29·384	28·71	60	39	49	w.	nw.	·01	·46
5	29·846	29·661	29·10	62	37	51	w.	calm	...	·11
○ 6	29·962	29·913	29·27	64	42	51·5	sw.	calm	·06	...
7	30·105	30·029	29·60	60	46	51	e.	calm	...	...
8	29·969	29·704	29·44	60	45	55	e.	e.	·10	...
9	29·478	28·929	28·85	55	46	51	sw.	calm	·52	·08
10	29·235	28·871	28·42	55	36	44	nw.	nw.	·01	·31
11	29·735	29·380	28·83	58	36	45	nw.	nw.	...	...
12	29·911	29·882	29·37	59	45	42	w.	nw.	·14	...
13	30·141	29·949	29·37	65	53	54	w.	calm	·02	·06
☾ 14	30·356	30·227	29·65	58	52	52	w.	calm	...	...
15	30·431	30·394	29·85	59	49	46·5	ne.	calm	...	...
16	30·429	30·317	29·87	55	48	51	se.	calm	...	...
17	30·310	30·278	29·77	55	41	51	nw.	calm	...	...
18	30·348	30·273	29·76	53	30	50	ne.	se.	...	...
19	30·321	30·182	29·74	53	27	43·5	se.	calm	...	...
20	29·951	29·795	29·43	54	37	43	sw.	calm	·13	...
21	29·815	29·804	29·27	54	32	40	w.	calm	·06	·11
● 22	29·626	29·480	29·22	55	39	42·5	s.	sw.	·20	·33
23	29·745	29·467	29·12	55	45	44	w.	w.	·26	...
24	29·737	29·679	29·11	57	39	46	s.	w.	·01	·45
25	29·739	29·248	29·21	56	43	45	s.	calm	·67	·08
26	29·535	29·295	28·70	57	32	45	sw.	w.	·02	·67
27	29·973	29·685	29·18	52	30	44	w.	calm	...	...
☽ 28	30·087	30·010	29·54	60	39	38	s.	calm	·25	...
29	30·149	29·900	29·35	59	31	48	sw.	w.	...	·14
30	30·245	30·086	29·70	55	46	42	se.	calm	·58	...
31	30·071	29·889	29·53	47	38	47	w.	e.	·04	·42
	30·431	28·871	29·26	65	27	47·5			4·05	3·58

Remarks.

London.—October 1. Fine: stormy with heavy rain at night. 2. Heavy clouds: rain: clear. 3. Hazy: rain. 4. Fine. 5, 6. Heavy dew: very fine. 7, 8. Foggy: fine. 9. Rain. 10. Rain: barometer unusually low: clear at night. 11. Clear and windy. 12. Cloudy: rain. 13—15. Hzy. 16, 17. Dry haze. 18. Cloudy and fine. 19. Hoar frost: very fine. 20. Frosty: rain. 21. Hazy: fine. 22. Showery. 23. Clear and fine: windy with rain at night. 24. Fine. 25. Stormy with rain. 26. Windy. 27. Fine. 28. Clear and frosty: rain at night. 29. Showery. 30. Foggy: heavy rain at night. 31. Hazy: rain.

Boston.—October 1. Fine: rain last night: rain P.M. 2. Cloudy: rain early A.M.: rain A.M. 3. Cloudy. 4. Cloudy and stormy: rain early A.M. 5. Fine: rain early A.M. 6. Fine. 7. Cloudy. 8. Fine. 9. Cloudy: rain early A.M.: rain P.M. 10. Fine. 11, 12. Stormy. 13. Rain. 14. Fine. 15—18. Cloudy. 19. Fine. 20. Fine: rain P.M. 21. Fine. 22. Cloudy: rain early A.M. 23. Fine. 24. Fine: rain P.M. 25. Fine: stormy night with rain. 26. Stormy. 27. Fine 28. Fine: ice this morning. 29. Cloudy: rain early A.M. 30. Cloudy: rain P.M. 31. Rain: rain early A.M.

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END OF THE SEVENTH VOLUME.

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